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BEAD MAKING IN ANCIENT SIND *

ERNEST MACKAY
MONK'S RISBOROUGH, ENGLAND

BY THE EXCAVATIONS in 1935-36 of the Expedition of the American School of Indic and Iranian Studies and the Museum of Fine Arts, Boston, at Chanhu-daro in the Nawabshah District of Sind, India, remains of the same culture have been revealed as in the ancient Indus cities Mohenjo-daro and Harappa. All three cities were populated by people of this Harappa culture during the period 3000–2500 B. C., and there is no evidence as yet of any previous occupation of these sites.[1]

A few unfinished beads were found during the excavations at Mohenjo-daro. Their number was so small, however, as to suggest that though bead-making was a craft practised in that city it was not carried on to any great extent, unless it were in parts of the city that have not yet been explored.[2]

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* This article comes to the JOURNAL from the American School of Indic and Iranian Studies and the Museum of Fine Arts, Boston, which jointly conducted a year's excavation at the archaeological site of Chanhu-daro, Sind, during the season of 1935-36. The license to excavate was the first granted by the Government of India under the amended Ancient Monuments Act, which now permits private archaeological excavation in India in accordance with specific rules and under the supervision of the Director General of Archaeology. The American School of Indic and Iranian Studies was planned by a committee appointed by the American Oriental Society in 1927 (see JOURNAL, Vol. 47, p. 358), and then later was fostered by the Committee on Indic and Iranian Studies of the American Council of Learned Societies. It was incorporated in 1934, and this excavation was initiated by the School. The School was fortunate in securing as field director of the excavation Dr. Ernest Mackay, whose long and varied archaeological experience includes six years of work at Mohenjo-daro. The expedition's share of the finds are now in the Museum of Fine Arts, Boston, which owns them permanently. This article is the first detailed scientific study to be published dealing with any of the finds of the excavation; brief general accounts by Dr. Mackay have appeared in the *Bulletin of the Museum of Fine Arts*, October, 1936, and *The Illustrated London News* for November 14 and November 21, 1936.—EDITORS.

[1] The term "Harappa culture" is preferable to "Indus Civilization," which is too comprehensive: other cultures have been and will be found in the ancient Indus valley.

[2] At least three-quarters of the site awaits examination and evidence of many crafts may well be found in the workshops in which they were carried on.

Quite early in the excavation of Chanhu-daro numbers of unfinished beads were unearthed, and later in the season, when we had removed the uppermost strata of the largest mound, we came across not only large numbers of incomplete beads but also the raw material from which they were made, and, still more interesting, the actual stone drills by which they were bored. In addition to providing other interesting material, Chanhu-daro has proved to have been a great centre of bead-making, from which industry it undoubtedly derived a great measure of its prosperity.

Curiously enough, very few finished hard stone beads were found. This can only be explained by the fact that the people of Chanhu-daro had more than once to desert their little city by reason of floods. The river Indus, which is now some twelve miles distant from the site, in former days was only three miles away, and during the actual occupation of the city it may have been nearer still and been a constant menace. The floods that imperilled the city on each occasion left their traces in the subsidence of the buildings; and eventually a more than usually destructive flood led to the complete desertion of the city, which lay derelict some five hundred years or more until its re-occupation by people of another culture, with whom we are not here concerned. The bead-makers, then, were several times forced to leave their city, and they must have taken with them all their finished stock, leaving only unfinished material behind, probably with the idea that they would return later to carry on their craft.[3]

Among the various beads made and worn by the people of the Harappa culture the long barrel-cylinder shape (Pl. I 8, top) was a favorite. Very fine specimens have been unearthed at Mohenjo-daro, some no less than 4.85 in. long and made of the finest translucent carnelian that it was possible to obtain.[4] No beads of this size, even unfinished, have been found at Chanhu-daro, where the longest specimen, which is yellow agate and unbored (Pl. I 8, third from top), is only 3.65 in. long. Nevertheless, most of the beads of this type were of a very respectable length and when finished must have commanded a large price.

It is not my intention to discuss the relationship of these beads

[3] We have evidence that a considerable interval elapsed before the city was reoccupied each time that it was abandoned.

[4] *Mohenjo-daro and the Indus Civilization*, p. 511; Pl. CLI, b.

with those found in other countries. Suffice it to say that very similar beads, both of agate and carnelian, have been found at Sumerian sites, contemporary in date with those from Chanhu-daro and Mohenjo-daro; but it is still uncertain whether Sumer imported beads of this kind or whether they were made there.[5]

From the great number of unfinished beads in various stages of manufacture that have been found at Chanhu-daro, we now know exactly how the finished products were arrived at. Quantities of the raw material, nodules of agate and carnelian, were found in various parts of the site (Pl. I 2, 5, 6), varying in size but averaging some 3 to 4 in. long. Each nodule had a rough wrinkled skin, of which small pieces had often been removed to show the color inside. But many were sufficiently translucent to reveal the color of the interior if held against a strong light. Often the color was far from uniform: some nodules were a muddy yellow with red or darker yellow cloudy areas, others were clear agate with occasional dark brown patches; and, judging from the reddened surface, certain of them had been roasted to intensify the color of the interior, a practice which is still common in India and other parts of the world.

These nodules of agate and carnelian had all been brought from a distance; no suitable colored stone is procurable in Sind. Most probably they had been brought by sea and then up the river to Chanhu-daro, possibly from the region of Rānpur in Ahmadabad, or from a site near the modern village of Ratanpur in the Rajpipla State, or, it may be, from localities in Kathiawar where various kinds of agate are still to be found.[6] The two former districts still produce agates and carnelians, which after being treated to improve their colors are sent to Cambay in the Gujarat Division of Bombay, where an extensive bead industry is still carried on.[7] It is more

[5] I am inclined to the view that these beads were obtained from India, for no unfinished specimens have yet been found in Sumer, though unfinished beads of other types are well known there. It is tempting to think that perhaps Indians actually practised their craft in Sumer as well as exported beads thither.

[6] Sir Edwin Pascoe in *Mohenjo-daro and the Indus Civilization*, p. 681.

[7] For an excellent article in which the various sites from which agates and carnelians have been obtained in historic times in India are discussed, see A. J. Arkell, "Cambay and the Bead Trade," *Antiquity*, Vol. X, pp. 292-305.

than likely that the raw material reached Chanhu-daro from these districts, perhaps through the river port of Broach.

In order to obtain as long a bead as possible, the nodule of agate or carnelian [8] was split along the longer axis to produce a number of slips or rods, square or slightly rectangular in section. At each end every slip has part of the original surface of the nodule from which it was struck, and this sometimes also remains along one side of the slip. To prepare the slips from a nodule by striking alone would have been a very wasteful, as well as difficult task. In actual fact, one or two grooves along the exterior angles of most of them show that the nodules were first sawn longitudinally and then cleaved into sections when the cuts were deep enough (Pl. I 8, bottom: the section is 3.22 in. long). Judging from the rounded bases of these saw-cuts, it was at first thought that a wire saw was used, together with a fine abrasive, emery or the like; indeed, this form of saw was used in Sind until only a short time ago for cutting up agate and similar hard stones. But it seems more likely that a toothless metal plate was used as a saw: it would have the advantage of making a straighter and more level cut, the angles of which would, of course, later become rounded by wear.[9]

After the rough blanks had been separated from the parent stone, they were worked by coarse flaking to a more even shape. The next stage was the removal of the sharp angles by finer flaking (Pl. I 8, second from bottom: 2.87 in. long), which was followed by more careful and minute flaking until the bead was almost, but not quite, round in section (Pl. I 8, third from top). When this stage had been reached, recourse was had to grinding, and the bead was finally shaped by rubbing it to and fro on a piece of coarse sandstone (Pl. II 13).

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[8] Most of the stone was agate. Very few nodules were carnelian, even then the red was far from uniformly distributed. None had the deep uniform translucent red that is seen in the long barrel-cylinder beads of Mohenjo-daro. Possibly this deep red was obtained after the bead had been shaped and bored.

[9] At Cambay, a plate-saw is used for cutting agates and the like, with, of course, the aid of an abrasive. For particulars of this, see Arkell, *op. cit.*, p. 297.

As iron was not then known in Sind, copper saws must have been used; owing to the softness of the metal, they would have been quite as effective with the aid of an abrasive. Bronze would have been no good owing to its greater hardness; though, of course, this alloy was well known and used for other implements.

Quite a number of these bead-stones were found, some of which had evidently had considerable use and others very little. It appears that the grooves in the more used stones were not prepared beforehand with the idea of shaping the bead, but were actually worn by the rubbing. These grooves all show a slight trace of polish, though no abrasive was used with them, for the stone was sufficiently gritty.

Some of these rubbing-stones are shaped like an animal, with what idea we do not know. No. 6 in Pl. II, which is 9.5 in. long and is made of sandstone similar to the ordinary bead-stones, is one of three that were found at Chanhu-daro; others of exactly similar shape have been unearthed at Mohenjo-daro. These animal-shaped stones have mostly been found broken, and the one illustrated had lost its tail which was thick and carried horizontally. Their backs are invariably grooved from side to side by the rubbing of beads across them.[10]

After the bead had been rounded, the rough ends were ground down flat, and before the final polishing the boring was commenced. In rubbing down the bead care had to be taken that its barrel-shape was preserved, and this was done by holding it so that only one-half of its length at a time was in contact with the stone. When this half of the bead had been ground, the other was done. The bead was apparently held in the hand during this process, which did not need particular skill and may have been entrusted to boy or girl labor.

Each end of the bead was then prepared for boring by first roughening its centre so that the drill should not slip. It is not quite certain with what kind of tool this was done, but certain ribbon-flakes terminating in a point (Pl. II 1-3)[11] were, I think, used for this purpose. This preliminary roughening is illustrated in Pl. III 1, which is a micro-photograph of one end of an unfinished long barrel-cylinder bead (diameter 0.3 in.). It will be noticed that the drill had a splintering rather than a true cutting action.

From examination of the unfinished hard-stone beads that were made by the people of the Harappa culture, it was at first thought

[10] The animal is thought to be a mongoose, but the large eye seems against this identification. These animal bead-stones were at first thought to be hones for household use, but the grooves are too deep and narrow to have been made with metal tools.

[11] From the left these measure 1.53 in., 1.4 in., and 1.5 in. in length. They are light gray and dark brown chert.

that copper or bronze tubular drills were employed to bore the holes. Beads that have been broken in the boring show at the break a slight polished core surrounded by a smooth circular ring. Numbers of examples have been found at Chanhu-daro and a few at Mohenjo-daro, all with this characteristic pimple in the centre of the break. Pl. III 5, showing this feature, is a micro-photograph of the other end of the bead illustrated in Pl. III 1 (diameter 0.3 in.).

That stone and not metal drills were used in boring the hard-stone beads of Chanhu-daro is now proved by a large number of stone drills being found there. Pl. II 5 illustrates a few of these drills, some broken and some perfect, all with the exception of the topmost photographed with the working end downwards. The perfect specimens average 1.5 in. long by 0.1 to 0.12 in. in diameter (Pl. II 5, left-hand bottom corner). The Director of the Geological Survey of India has kindly examined two of these drills for me and reports on them as follows: "Two specimens, described as 'Drills,' consist of chert, containing a little magnetite; the hardness of the specimens is 7. Such specimens do not occur in nature in this rod-like form; they have apparently been worked into shape from material likely to occur in any of the Archaean rocks of India."

The business end of these stone drills is rounded, with a slight depression in the centre. A micro-photograph (Pl. III 8) of the end of one (greatest diameter 0.12 in.) clearly shows the concentric markings formed by an abrasive in its rotation against a hard substance, or, alternatively, the rotation of a hard stone against it. All these drills, whether black or dark brown in color, were made by roughly flaking the stone into a rod-like shape (Pl. II 4: 1.5 in. long), and then grinding them in much the same way as the beads. They are never of the same diameter right through, but narrow slightly just above the working end and then thicken towards the butt, which more often than not has slightly facetted sides, doubtless to prevent its turning in the handle or chuck in which it was fixed. It might not at first appear possible to use so brittle a drill with the hardness of 7 against a material of similar hardness, such as agate or carnelian. By itself the drill would have made little or no impression on these stones, but the use of a fine abrasive with it, such as emery or crushed quartz, would entirely alter matters. Dr. C. H. Desch, Director of the National Physical Laboratory, Teddington, England, to whom I submitted some of these drills for experiment, writes as follows: "I think that the depression at the

end of the drills (see Pl. III 8) is intended to hold the abrasive under the drill and prevent it from escaping. I mounted one of the stone drills in a small Archimedean brace, which I held vertically. The action would be just the same as that of a bow drill. Using 120-mesh emery and water, I found that it took about 20 minutes to drill a depth of a millimetre in one of the rough pieces of carnelian. A small depression must have been made to locate the drill, as on a flat surface the drill wanders around before getting to work. The drilling is certainly quite practicable. Failing emery, possibly even sand may have been used. The wear on the drill is very slight."

The depression spoken of by Dr. Desch as a necessary preliminary for guiding the drill is that already mentioned as occurring on the ends of many of the unbored beads (Pl. III 1), and was probably made with a tool like those seen in Pl. II 1-3. I had early arrived at the opinion also expressed by Dr. Desch that the slight cavity (Pl. III 8) in the points of these drills might serve to hold some of the abrasive; but if this were so, then it is a moot point whether the cavity was purposely hollowed out or was caused by the work that the drill had to perform. There is also the difficulty that the cavity, if it had held abrasive, would hardly have produced the effect in the beads of the slight pimple that is commonly found in those beads that were broken during boring (Pl. III 5). No finished drill has been found without this cavity, and it is reasonable to assume that it was purposely contrived. Its presence led to the supposition that the drills which we first found were unfinished beads, but their likeness to one another in the shallowness of the hole soon changed my views.[12] The slight narrowing of the drill just above the point was doubtless very useful in holding water as well as the abrasive, and thus preventing undue wear of the point.

The implement used for holding the drill was most probably worked with a bow. The bow-drill is in common use at the present day in India both for stone and wood. We must, however, not overlook the possibility that a pump-drill was used, as this tool also is well known in India. But whether the bead revolved or the drill itself is uncertain, nor do we yet know whether the beads were

[12] Mr. Horace Beck informs me that a specimen of one of these drills has been found at Ur; until he saw the drills from Chanhu-daro he had thought it to be an unfinished bead.

drilled in a horizontal or vertical position.[13] I am inclined to think that the bead was horizontal; owing to the difficulty of keeping a very long barrel-cylinder bead in exact alignment with the drill, it was probably inserted in a very deep wooden chuck, and perhaps also cemented in to prevent it from shifting. Even if this were done, the boring might occasionally be out of the straight, owing to faulty bearings.

Every hard stone bead, of whatever size or shape, was bored from both ends so that the drill-holes met in the middle or thereabouts. The holes are never of the same diameter throughout, but definitely narrow from the ends towards the middle owing to a certain amount of play either on the part of the bead or of the drill as the boring proceeded. This play was perhaps not a disadvantage as it permitted the easy supply of water and grinding material, which a very tightly fitting drill would prevent. In most cases the boring was carried out with success; in few of the beads did the holes fail to meet more or less accurately in the centre, though some of the finished specimens from Mohenjo-daro must have been exceedingly difficult to thread.

A very fine abrasive must have been used with the drills, for to the naked eye the bore-holes actually look polished, and in this respect they are far superior to the bore-holes made with diamond drills in India today.[14] In my work at Mohenjo-daro I thought that the holes were polished after the boring was finished, but broken beads from Chanhu-daro clearly show that no other finish to the holes was necessary.

Whereas at Cambay the boring is done last, several specimens show that at Chanhu-daro this process was completed before the bead was ground into its final shape and the traces of flaking removed. This was probably because of the fact that there were so many accidents in the process of drilling owing to unseen fissures in the stone. The beads were then given a final polish and were ready for the market.

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[13] On the Gold Coast, long beads are, or were, set in a vertical position encased in a block of wood while being bored with the aid of a bow-drill. For this information I have to thank Mr. Henry Balfour, who has this particular appliance on exhibit in the University Museum, Oxford.

[14] A. J. Arkell, *op. cit.*, p. 297. Mr. Arkell does not state how the diamond-headed drills are used in boring the modern Cambay bead, but we may assume that the drills are worked with a bow.

It is not yet known how the very long beads of Channu-daro were polished; but whatever the process employed it was very successful—even the ends of the beads were rounded off so that they should not cut the string. At the present day in India, a large number of beads are put into a leather bag with fine emery and carlinian powder and the bag is pulled to and fro between two men until the desired polish is acquired.[15] This technique may be effective with the shorter varieties of beads, but it would, I imagine, be hardly suitable for very long ones owing to their liability to break. We know from some of the pottery figurines, as well as from actual finds at Mohenjo-daro, that the long barrel-cylinder beads were frequently strung together and worn as girdles. They were, moreover, valued so highly that when broken they were often repaired, or the broken ends were ground down so that they could be used as shorter beads.

A large number of unfinished disc-shaped beads[16] also were unearthed at Chanhu-daro. The blanks for these beads were segments of the long slips prepared for the barrel-cylinder beads. Whether square or slightly rectangular in shape (Pl. I 1), these blanks were bored from either side with the aid of chert drills (Pl. II 1-3), the conical holes meeting in a minute hole in the centre of the bead.[17] The bead was then carefully flaked to round it off (Pl. I 4), a process followed by grinding, either by rubbing each bead separately on a stone or by threading a number together and rolling them to and fro on a slab. I am rather doubtful, however, about the latter suggestion; the interiors of the holes of the unfinished beads are very sharp and would tend to cut any thread passed through them (Pl. I 7).[18] Plate III 2 shows a section of one of these beads after it had been roughly bored (diameter 0.58 in.; height 0.29 in.), and No. 3 on the same plate shows the rough-

[15] Arkell, *op. cit.*, p. 297.

[16] Of the Convex Bicone type, No. A. I. e, of Beck's classification in *Archaeologia*, Vol. 77, Pls. II, III.

[17] The same type of bead was bored by a similar method at Hierakonpolis, Egypt, in Old Kingdom times; but the flint tools that were used were smaller: Quibell and Green, *Hierakonpolis*, II, pp. 11, 12. Very similar flint tools found by Campbell Thompson at Abu Shahrain in Mesopotamia were used, I think, for the same purpose: *Archaeologia*, Vol. 70, p. 119, Pl. IX. I suspect that the microliths found on so many sites in Sumer were used in bead-making.

[18] The illustrations in Pl. I 1, 4, 7 are about three-fourths of natural size.

ness or splintering of the boring (diameter 0.58 in.). What kind of tool was used to give a final polish to the conical holes of this type of bead is not known; it must have been made for the purpose, for the polished holes of the finished beads retain the original biconical shape.

It is not my intention to discuss separately all the varieties of beads of the Harappa culture found at Chanhu-daro, but several of the rarer types may well be mentioned here. Just inside the doorway of a bead-maker's house a mass of beads was found (Pl. II 14) that was possibly once contained in a basket which had long since perished. These beads, which are extraordinarily small and all of the same cylindrical shape, have become concreted together by salt into a mass, which rests on a small copper dish that cannot be separated from it without disturbing the beads. This dish appears to be a scale pan, of which we have found many specimens both at Chanhu-daro and Mohenjo-daro, and it seems likely that these minute beads were sold by weight after being strung. Judging from the floor-level of his workshop the bead-maker had been established there for some time. Another mass of beads of the same type was found in a small pottery jar at another part of the site. Mr. Horace Beck, to whom I have submitted samples, writes as follows: "The smallest bead which I have measured from Chanhu-daro is 0.0268 in. in diameter, which means 37.3 to an inch. It is stated that some specimens from the Harappa site are as small as 0.021 in., but the smallest that I have measured was 0.025 in. The perforation of the Chanhu-daro beads is approximately 0.01 in. An average of the perforations of the six beads measured was 0.0098, the maximum being 0.0107, the minimum 0.0083. After carefully examining a modern watchmaker's drill which is capable of drilling holes 0.01 in. (0.25 mm.) in diameter, it is difficult to believe that either the drill or the bead could have been held by the hand, so I suggest that some form of lathe or jig must have been used."

Mr. Beck continues: "The only stone beads yet discovered which compare with those from Chanhu-daro are those from Harappa mentioned above and a series from Ur. At Harappa great numbers were found, and they vary considerably in size; at Ur also great numbers of similar white beads were found, but they were in no cases as small as the Chanhu-daro beads, and mostly a great deal larger. They are identical with larger beads of the series from

Harappa.[19] The smallest steatite beads from Ur measured 0.04 in. in diameter. The Ur beads were associated with small beads of carnelian and lapis-lazuli and are thought to have formed part of a girdle. No beads of different materials appear to have been associated with the Indian steatite beads.[20] Only one find of steatite beads has been made at Ur, and the carnelian and lapis-lazuli beads show a different technique, so it seems reasonable to suppose that the steatite beads at Ur were imported."

Every one of this type of bead was bored, and, as the illustration (Pl. II 14) shows, they had once been strung together. How such minute beads could be bored puzzles Mr. Beck and myself. Analysis shows that they are made of steatite, but whether from the actual stone or from a compressed steatite powder has not yet been ascertained. Their varying lengths may come from being made as long tubes which were broken or cut up into short segments (Pl. III 6, height 0.58 in., greatest diameter 0.46 in.; III 7, greatest diameter 0.46 in.). I first thought these beads were made of a steatite composition, and were shaped by painting this composition round a smooth fibre or hair, which subsequent firing removed, but the following report by Drs. F. A. Bannister and G. F. Claringbull, of the Mineral Department of the British Museum (Natural History), leaves room for doubt: "The beads submitted to us are small hollow cylinders which vary in external diameter from ½ to 1 mm. and in length from ½ to 1½ mm. When freed from loosely adherent soil they are pure white and very hard. Nearly every bead is furrowed along its length spirally, always in a clockwise direction. It is not uncommon to find two beads joined together with the spiral furrowing running continuously and in perfect alignment from one to the other. The two beads are connected by a thin layer of friable soil which can be readily removed with a needle. We have carefully examined alternative views of how these tiny beads were made, but so far have been unable to reconcile all our observations with any one theory. The physical and chemical data are conclusive, however, as to the material from which these beads were made. Their specific gravity is 2.85 and the refractive index of crushed fragments is 1.60. The beads can only be fused in the electric arc, and then

[19] Report on Harappa Beads, 1934, but not yet published, by the Indian Archaeological Survey (H. B.).

[20] Correct for the beads from Chanhu-daro (E. M.).

yield glassy spheres with a lower specific gravity 2.72. Magnesium and silicon but no other elements could be detected with the quartz spectograph, and X-ray powder photographs show that the beads are steatite that has been completely dehydrated and heated to a temperature exceeding 1200° C. X-ray photographs have also revealed that much larger beads from the same locality (India) and Egyptian scarabs also consisting of baked steatite have only reached temperatures up to 1000° C."

This report seems to indicate that the beads were in fact made as long tubes, and the spiral furrowing referred to suggests that these tubes were shaped by squeezing a composition through an aperture; though how tubes rather than solid rods were produced it is difficult to see. Where two beads were found almost united and with their markings continuous, it seems that they must have passed after one another through the same apparatus.

Unfortunately, it is not possible to ascertain whether these beads were made of a paste or cut from the solid stone, for Dr. Bannister tells me that after the very high temperature to which these beads had been subjected anciently, all evidence of their being made of a composition of steatite would have been lost. At any rate these beads show craftsmanship of the highest order combined with extraordinarily good eyesight.[21]

A type of bead that is very common in all the cities of the Harappa culture is the wafer bead,[22] which varies greatly in size and also in the magnitude of the hole that perforates it. The larger beads of this type were generally made of steatite, which was shaped as a long rod that was subsequently cut up into thin segments, apparently by means of a saw. Most of these beads now show no traces of glaze or color.

Certain unfinished beads of this type deserve especial mention on account of their small size. These are thin, flat plates of dark brown steatite, irregular in shape and averaging 0.1 of an inch across. Each of these little unfinished beads is perforated by a minute hole, the diameter of which, according to Mr. Beck, corresponds very closely to the perforations in the tiny tubular beads

[21] These particular beads can be regarded as finished. Though the majority are now white, Beck has in some detected traces of a broken-down silicate glaze, probably originally blue.

[22] Classed by Mr. Beck as "Disc-beads," A. 2. b., *Archaeologia*, Vol. 77, Pls. II, III.

already referred to. A specimen bead (greatest width 0.15 in.) is shown in Pl. III 4. Very noticeable in the illustration is the irregularity of the hole, which looks to me to have been made by a fine metal point twirled between the fingers—an easy process with a soft stone like steatite.

Mr. Beck states that "these plates are either unfinished beads in the process of manufacture, or beads that have for some reason been discarded when only half made. The diameter of the holes corresponds very closely to the perforations in the (tubular) beads. The thickness of the plates varies from 0.012 to 0.02 in. in the specimens measured. It is an interesting fact that they are made of steatite which does not appear to have been treated by burning."

These unfinished beads were apparently made by first cutting up steatite into thin plates which were rubbed down to the requisite thinness and then cut into blanks approximating in area to the size of the beads required. It seems certain that the original plates were thus broken up before the process of boring, for otherwise there would have been a tendency for breakage to occur *through* the holes, instead of irregularly around them. The final rounding off of these beads could have been effected by stringing them tightly on a fibre or hair, and rubbing the uneven rod thus formed smooth all round. This was very likely done after the perforated blanks had been subjected to heat to harden them, for steatite heated at 900 C. for the space of one hour can be taken from 1 to 7 of Moh's scale of hardness, and is, moreover, whitened in the process.[23]

Mr. Beck is inclined to think that these little plates are the beginnings of the minute tubular beads already described. With this, however, I am not inclined to agree. For one thing, the thickness of the perforated plates is smaller, though certainly not by much, than the lengths of the tubular beads. I cannot but think that it was intended to make wafer beads from these plates; we have, in fact, unearthed at Chanhu-daro small finished waferbeads, ranging in size from 0.08 to 0.1 in. in diameter, white in color and pierced with a minute hole, that seem to have been made in this way.

Fifteen specimens of the decorated type of carnelian bead have been found at Chanhu-daro, some of which are illustrated in Pl. II 7-12. Several were broken or defective beads, and it is certain

[23] For particulars of this treatment, see F. A. Bannister and H. J. Plenderleith in the *Journal of Egyptian Archaeology*, Vol. XXII, pp. 3-6.

that beads of this technique were made in some, at least, of the bead factories of the city. The design on No. 7, which is a perfect specimen, 0.6 in. long, and, moreover, a darker red than is usually found in this type of bead, is unknown at Mohenjo-daro; but beads of similar design, though different in shape, have been unearthed at several sites in Sumer,[24] where they have been dated to approximately the same period. Bead No. 9 is of particular interest as its design is in black on a whitened ground. Only one specimen of this particular technique, as yet unpublished, has been found at Mohenjo-daro, and another was found by Sir Leonard Woolley loose in the soil of the Sargonid stratum at Ur.[25] The design on this bead, which is incomplete and now measures 0.74 in. long by 0.58 in. wide by 0.17 in. thick, is very similar to that on certain beads found at Ur and two at Mohenjo-daro.[26]

The design on No. 10 in Pl. II varies very little from one seen on beads from Ur,[27] and the patterns on Nos. 8, 11, 12, are quite common on decorated carnelian beads from Tell Asmar, Kish, and Ur in Mesopotamia, and Mohenjo-daro and Harappa in India. The great similarity of the patterns on these widely scattered beads strongly suggests that they originated in one centre. It is also significant that in most cases their shapes are alike, and that they occur in levels contemporary with one another. It was only a short time ago that the manufacture of decorated carnelian beads ceased in India, and I feel convinced that it was that country that first exported them, early in the third millennium B. C., and that afterwards the craft was practised in several countries.[28]

I have before remarked that very few unfinished hard stone beads have been found at Chanhu-daro; and carnelian itself is very rare, even among the large number of nodules that formed the raw material from which the beads were made. A light or deeper

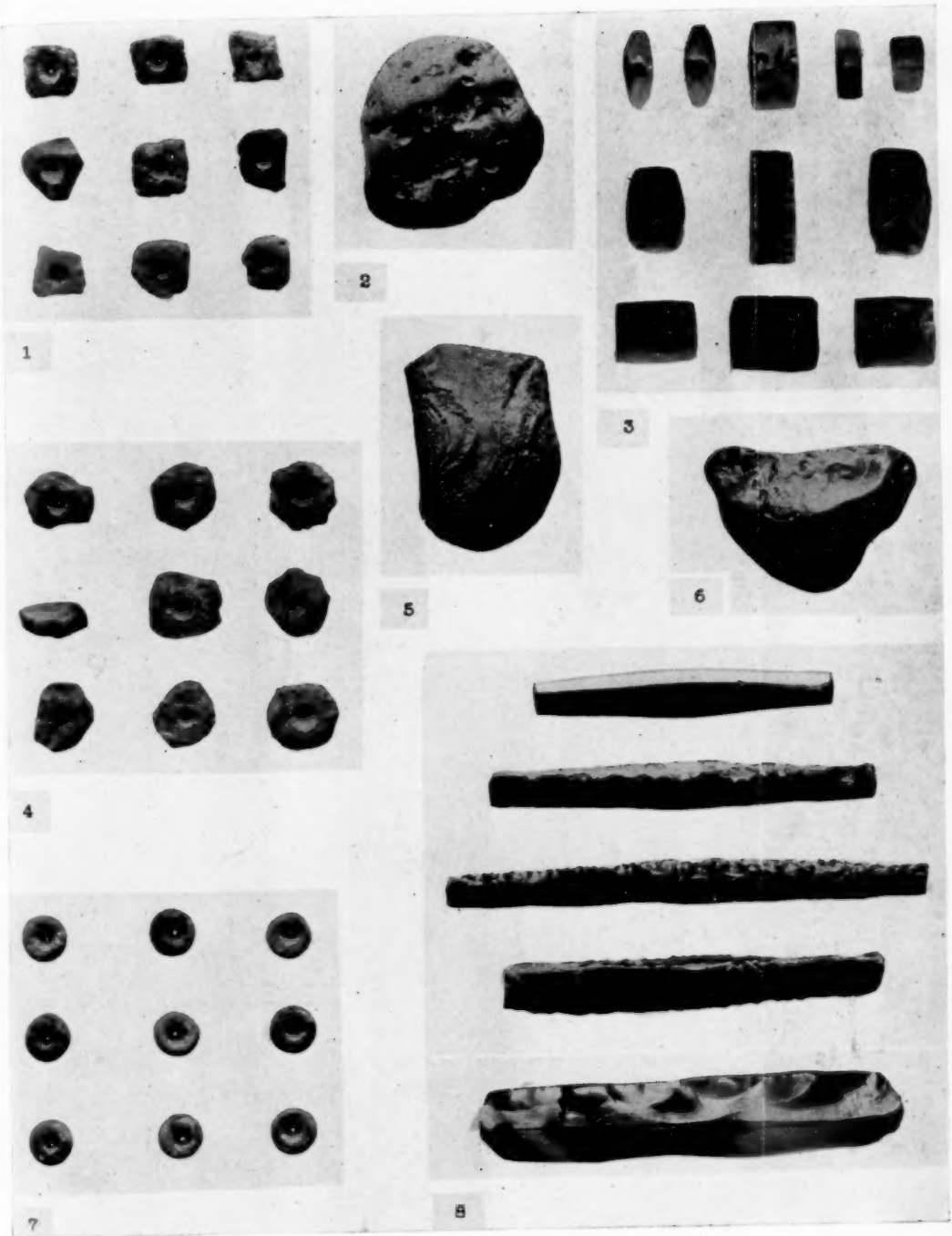
[24] Frankfort, *Iraq Excavations of the Oriental Institute*, Chicago, 1932-33, Fig. 29; Woolley, *Royal Cemetery*, Pl. 133.

[25] *Antiquaries Journal*, Vol. XIV, p. 559. This technique is also seen in beads from Taxila. Specimens of it have been unearthed at Tell Asmar and in Baluchistan, for particulars of which see Beck, *Antiquaries Journal*, Vol. XIII, pp. 384-398.

[26] *Mohenjo-daro and the Indus Civilization*, Pl. CXLVI, 45. The second specimen will be published shortly.

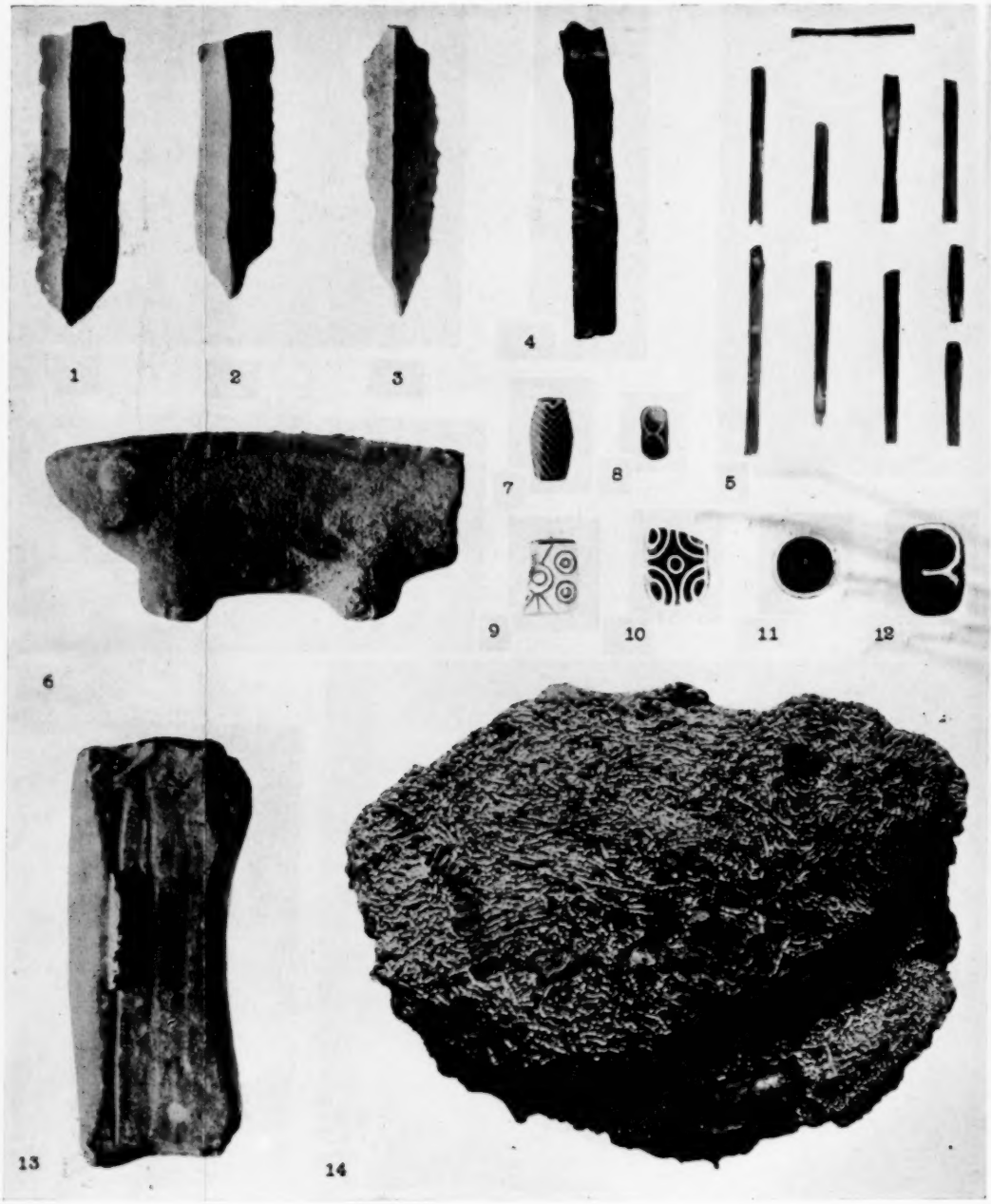
[27] Woolley, *Royal Cemetery*, Pl. 133.

[28] They have not yet been found in Egypt, but specimens made at a later date come from as far west as Russia.

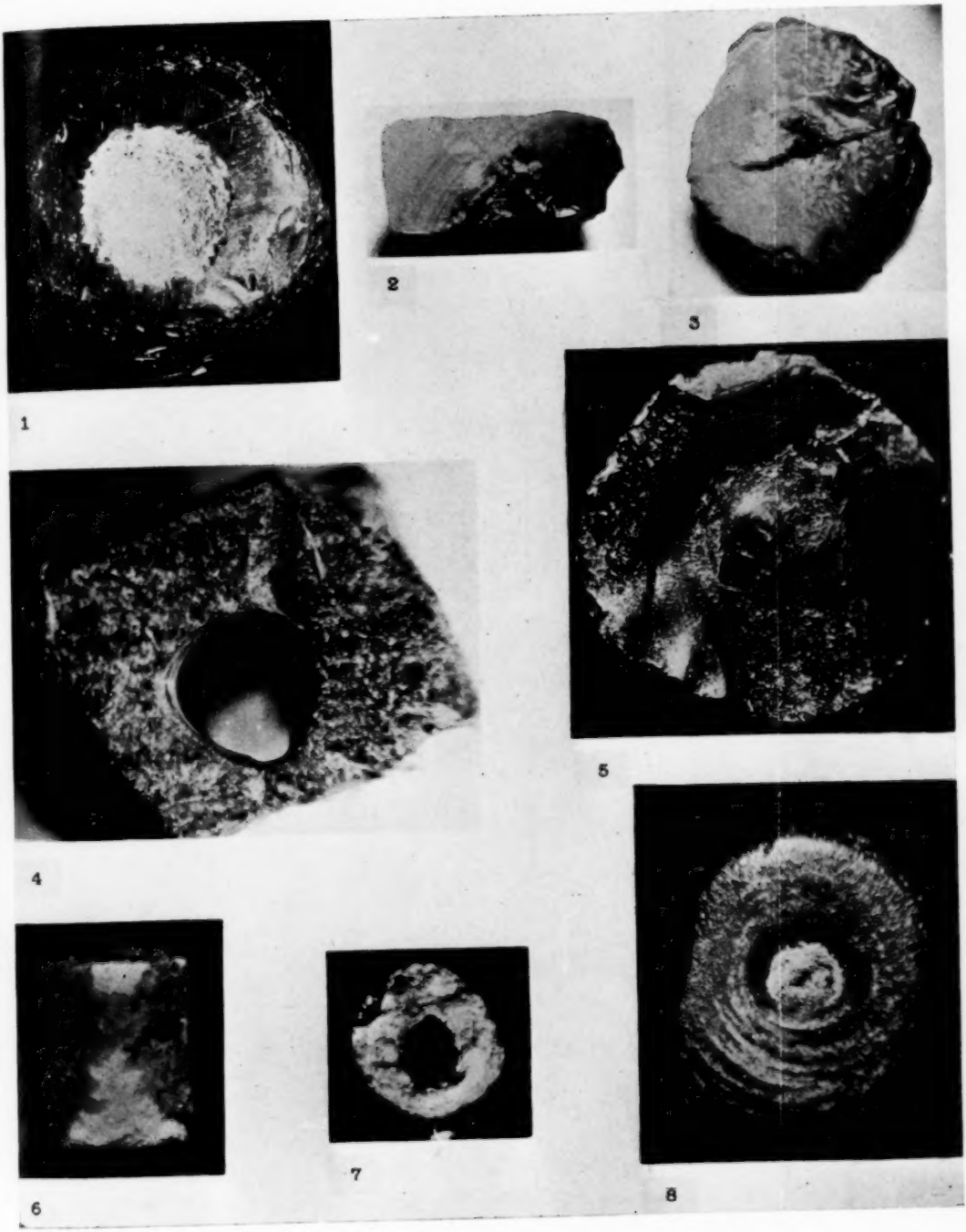


Unfinished Beads from Chanhu-daro, Sind

PLATE II



Beads and Bead-Drills from Chanhu-daro, Sind



Micro-photographs of Beads from Chanhu-daro, Sind

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colored yellow agate, sometimes very cloudy, was the material that was commonly used at Chanhu-daro: but it well known that the colors of agates may be changed by heat, and it is possible that many of the beads, especially the long barrel-cylinder type, were artificially turned red just before, or even after, they received their final polish.[29] Only one bead of the barrel-cylinder type has been found at Chanhu-daro of the uniform deep red so common at Mohenjo-daro and evidently greatly admired. Another bead was yellow at one end, red at the other and dark brown in the centre.

Though many unfinished hard stone beads have been found in Egypt, a country which was certainly a great centre of bead-making, and which from very early times worked more varieties of stone than any other land, it is surprising that except for the rough flint drills reported from Hierakonpolis, nothing approaching the finely finished stone drills lately found at Chanhu-daro seems to have been used. Similar drills may, however, yet be reported from Egypt; it is quite possible, indeed, that they have already been found and identified as something else.[30] Beck now reports one specimen from Ur; and though I have not yet seen them, Dr. Campbell Thompson has drawn my attention to certain obsidian rods which he found at Abu Shahrain, which from their appearance could well have been used as drills, although they differ somewhat in shape from those from India.[31]

In conclusion, I should add that hard stone beads of several other shapes than those mentioned above were made at Chanhu-daro (Pl. I 3). The technique of their manufacture, however, presents no features other than those already described in this review of the bead-maker's craft in ancient Sind.

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[29] It is usual at the present day to alter the color of agate by roasting it in its nodular form at the place where it is mined. Agate can also be converted into carnelian by boiling the stone in a solution of iron: Arkell, "Cambay and the Bead Trade," *Antiquity*, 1936, p. 302.

[30] I would suggest kohlsticks.

[31] *Archaeologia*, Vol. 70, Pl. IX, Row 8, pp. 120. I have not been able to examine these rods, owing to alterations in the British Museum.

THE AORIST IN BUDDHIST HYBRID SANSKRIT

FRANKLIN EDGERTON
YALE UNIVERSITY

§ 1. THIS IS a preliminary draft of a chapter in the Grammar of Buddhist Hybrid Sanskrit.[1] It is based almost wholly on the Saddharmapuṇḍarīka and Lalitavistara, supplemented by a few stray observations from other texts. Most of the materials are taken from the verse parts of those two works, since the prose, at least as presented in the editions, yields little.[2] All forms quoted in this

[1] For general orientation see *BSOS* 8. 501-16; for a similar preliminary account of nouns of the *a*-declension see *HJAS* 1. 65-83. For an account of the metrical peculiarities of the verses of SP see *KSCV* 39-45 (but this article, no proof of which was sent to me before printing, contains some errors and many misprints, some so serious as to result in unintelligibility).—The following abbreviations are used in this article (it is hoped that others will be self-explanatory):

AMg = Ardhamāgadhī
Ap = Apabhraṃśa
BSkt = Buddhist (Hybrid) Sanskrit
BSOS = Bulletin of the School of Oriental Studies, London
Calc. = the Calcutta (Bibliotheca Indica) edition of LV.
Geiger = Wilhelm Geiger, Pali (Strassburg, 1916)
HJAS = Harvard Journal of Asiatic Studies
Hoernle = A. F. R. Hoernle, *Manuscript Remains of Buddhist Literature found in Eastern Turkestan* (Oxford, 1916). Contains editions, by Lüders and Thomas, of several fragments of Kashgar mss. of SP.
KSCV = The Kuppuswami Sastri Commemoration Volume (1936), 39-45 (see above).
LaVP = La Vallée Poussin, *JRAS* 1911, 1067-77 (edition of another fragment of a Kashgar ms. of SP).
LV = Lalitavistara (ed. Lefmann unless otherwise specified by "Calc.").
m. c. = metri causa
Mhv. = Mahāvastu (ed. Senart).
O = readings of mss. of the Kashgar recension of SP quoted by this designation in the critical notes to Kern and Nanjio's edition.
Pischel = R. Pischel, *Grammatik der Prakrit-Sprachen* (Strassburg, 1900).
SP = Saddharmapuṇḍarīka (ed. Kern and Nanjio unless otherwise specified by "WT").
WT = Wogihara and Tsuchida's edition of SP (Tokyo, 1934-5).

[2] Perhaps if we knew its original form it would yield more; cf. *BSOS* 8. 507. Our printed texts of SP and LV are based mainly (in the case of

article are taken from verses, except those specifically identified as prose.

§ 2. *Metrical license.* For the purpose of this paper (see *KSCV* for details) it will suffice to recall that in verses any short vowel in an open syllable may be lengthened, and any long vowel shortened, metri causa; and that all final syllables ending in a vowel are open, since consonant clusters at the beginning of a word (tho regularly written in Sanskrit fashion) were always pronounced Prakritically, as single consonants. Hence e. g. the Prakritic aorist ending *ī* (Skt. *īt*) is constantly shortened to *i* when the meter requires a short syllable; and the semi-Prakritic ending *iṣu* (3 pl.) may appear as *iṣū*, *īṣu*, or *īṣū* according to metrical requirements.[3]

LV wholly) on Nepalese tradition. What we know of the Central Asian or "Kashgar" recension of SP suggests that its prose was much more Prakritic, and presumably closer to the original text. It is regrettable that we have no complete edition of it. Kern and Nanjio quote some readings of fragmentary Kashgar mss. by the collective symbol "O"; other fragments have been published by LaVP. and by Lüders and Thomas in Hoernle (see note 1). We shall see that these fragments present some Prakritic aorist forms where the printed text of SP has regular Sanskrit forms.

[3] Altho the latter would be equally satisfied by use, before an initial consonant, of the Sanskrit ending (*iṣ-*)*uḥ* (or its sandhi equivalent), instead of (*iṣ-*)*ū*, for (*iṣ-*)*u*, this very rarely occurs in forms that are otherwise Prakritic or semi-Prakritic.—The freedom with which vowels are lengthened and shortened according to metrical requirements sometimes leaves us in doubt as to whether a long or a short vowel was normal in a given form; that is, whether the one or the other is due to metrical necessities. If either is clearly established in prose, this of course settles it; but frequently the prose has neither. In that case we must try to find the form in metrically indifferent positions. Such positions include the first half of anuṣṭubh (śloka) pādas, and in SP the final syllables of all pādas. On this basis we can say with some confidence that the common 3 sg. aorist form of the Skt. *iṣ*-type (*deśayī* etc.) had *ī*, for this is often found at the end of lines in SP, and short *i* seems almost never to occur there (I have noted only *abhyayāci* SP 191. 8; to be emended?), nor in the first half of anuṣṭubh pādas, nor in prose (unless we accept *vyohārāpayi*, the v. l. of O in SP 109. 8, prose, for ed. *saṃsthāpayati*; in SP 190. 16 *āsi* = Skt. *āsīt* [which formally belongs to the aorists in our language, § 22] is printed at the end of a line of verse but we should doubtless adopt the v. l. *āsī*). On the other hand, and quite surprisingly, the *s*-aorist *abhūṣi* (root *bhū*) seems to have that form when the meter is indifferent. The rare *abhūṣī* occurs only in positions where meter requires a long: SP 383. 11; LV 164. 3 (at the end of a line but in a meter requiring a long final and otherwise

§ 3. *Third singular forms generalized.* As remarked in *BSOS* 8. 504, any third-singular verb form is freely used in our language with subjects of other persons and numbers: most commonly third plural, but not infrequently first and second singular. (First and second plural subjects are rare. Dual forms of both noun and verb, while not unknown, are more often, in the verses, replaced by plurals, as in Pali and Prakrit.) To some extent this phenomenon is known to Prakrit. In the optative it is general Prakrit (Pischel § 459). In AMg. (the only Prakrit treated in Pischel which has the aorist) it is also common in aorist forms (ib. §§ 515-7). Our dialect carries it much further, to all verb forms; but so far as the aorists are concerned it does not differ from AMg. usage. In Pali the usage seems to be recorded only of *atthi* = Skt. *asti* (Geiger § 141).

§ 4. *Dialect relations.* The aorist is extinct in most Prakrits and in Ap.; of all the dialects treated by Pischel it is really alive only in AMg. It is however also fully alive in Pali. In general our language shows great resemblance in grammar to AMg., and almost none to Pali. In its aorist forms this is less strikingly true; there are points on which it resembles Pali more than AMg. However, it also differs from both at times. Indeed it is becoming increasingly clear that the protocanonical Prakrit, which underlies Hybrid Sanskrit, was a quite definite dialect standing on its own feet, and independent of any Prakrit known to the grammarians (and *a fortiori* of Pali).

§ 5. *Augment.* In the verses the augment is omitted in the majority of forms. Even the prose contains unaugmented preterites, some of which would otherwise be regular Sanskrit forms.

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showing lengthening there m. c.) and 167. 15 (at the end of a line of another meter like the preceding, cf. *sukhenā* 168. 17 with *ā* m. c. at the end of a pāda). Tho *abhūṣīt* occurs in prose according to the Kashgar ms. of Thomas (Hoernle 133 f.) six times where the edition (SP 327. 2, 5, 7, 328. 1, 3, 5) has *abhūt* or *abhūvan*, *abhūṣī* (without final *t*) cannot be proved to occur in prose (to be sure in two out of the above six cases Thomas' ms. reads *abhūṣī*, but since in the same context the other four read *°ṣīt* we must probably follow Thomas in regarding *°ṣī* as a mere corruption).—In LV, with its much more complicated meters, occurrence at the end of a pāda does not have the same significance as in SP. Many of the LV meters require a long final syllable and often show metrically lengthened vowels in that position, as noted just above.

Thus *prati-jñāsiṣam* LV 418. 15, 19, which is the more surprising because it occurs in a very ancient canonical prose passage found in almost identical language in Pali (Mahāvagga 1. 6. 27, 28); and in the Pali form the verb (*paccaññāsiṃ*) is augmented! Which may, possibly, indicate that our text has kept an original augmentless form which even Pali provided with an augment in its recension. In general, to be sure, our prose shows few augmentless forms in the editions. But probably the original showed more. For the Kashgar recension has them at times where the editions, with the Nepalese mss., have other forms. Cf. *udgṛhṇīṣu dhārayiṣu paryāpuniṣu*, v. l. of O at SP 181. 9, and *saṃprakampayiṣu* Lüders (Hoernle 152) for *saṃprakampitāḥ* SP 270. 12 (others § 20, below). On the other hand an augmented aorist is used in a *mā* clause, where Sanskrit regularly omits the augment: (*mā . . .*)*akārṣuḥ* LV 89. 10 (prose). And in the verses occur forms which are un-Sanskritic and nevertheless have the augment; especially from the roots *vac* and *dṛś* (*avacī, adṛśī*, etc., to some extent paralleled in Pali), but also from other roots.

I. Descendants of the Sanskrit *iṣ*-aorist

§ 6. *Stem form and normal endings.* From the *iṣ*-aorist of Sanskrit is derived the commonest aorist type of our language, which seems to have been indefinitely productive in protocanonical Prakrit. It generally uses the ending *ī* (for which *i* occurs as a substitute, but almost exclusively m. c.), originally 3 sg. (Skt. *īt*), but here generalized for all persons and numbers. Fairly common also are distinctively 3 pl. forms corresponding to Skt. *iṣuḥ* (here *iṣu, iṃsu*, etc.; details below). Other endings occur rarely in the verses.— The ending *īt* is rare in Prakritic forms; *śraddadhīt* SP 113. 10, and in prose *prābhañjīt* SP 159. 1, based on the BSkt. and Pali present *-bhañjati* (e. g. LV 175. 17).

§ 7. All these endings are regularly added not to the root, but to the theme of the present tense, minus the thematic vowel (*a*). Such forms are extremely common in the verses, tho rare in prose. As is implied by this formulation, I believe that these Prakritic aorists are always based on a thematic present.[4] The same condi-

[4] For *chini* "cut" LV 165. 22, which can only be a 3 sg. aorist to the root *chid*, I have not yet found a parallel present **chinati*. But the exception may be said to prove the rule. At least it is, as far as I can see,

tion holds in Pali and AMg., tho the definite relationship between *iṣ*-aorist and *a*-present, which I believe I have discovered here and which I think probably holds good in Pali and AMg. also, seems not to have been formulated. Examples: *paśyiṣu* SP 15. 5 (*paśyati*). —*cańkramī* LV 368. 16 (*cańkramatas*, 3 dual pres., SP 460. 2, prose).—*kṣipī* LV 92. 16 (*kṣipati*).—*adhyeṣi* LV 416. 3 (*adhyeṣati* " applies for instruction to," common in BSkt., = Pali *ajjhesati*).— *gacchiṣu* LV 74. 6 (*gacchati*).—*jahī* LV 134. 17 (*jahati* = *jahāti*). —*avaci* LV 370. 14 (cf. *vaced* 3 sg. opt. SP 258. 4, and Pali *avacaṃ* etc.).—*paribhuñji* LV 387. 7 (*bhuñjati* " U+ " in Whitney, *Roots*; frequent in BSkt.).—*śruṇi* " heard " SP 90. 4 (*śruṇe*[*t*], opt., SP 252. 6; *śruṇā*, m. c. for *śruṇa*, 2 sg. impv., LV 94. 12; *śruṇanti* LV 233. 10, etc.).—*nirmiṇi* LV 219. 19 (*abhinirmiṇeyaṃ* 1 sg. opt., SP 196. 7, 9; to Skt. *-minoti*, altered to *-minati*).—*kurviṣu* SP 10. 6 (*kurvati*, frequent for Skt. *karoti*, e. g. SP 28. 8, 125. 14; cf. *BSOS* 8. 515). And many others. In prose (ending *īt*), *prābhañjīt*, see § 6.

§ 8. From passive stems (which to be sure, in BSkt. as in Pkt., are quite as apt to have active endings and to be treated in all

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impossible to derive *chini* from any other (that is from any heretofore recorded) form of the root or present stem, Sanskrit or Prakrit. The form actually compels us to assume a **chinati*, which may yet turn up somewhere. This would be a blend of Skt. *chinatti* with the regular Pali-Prakrit (and BSkt.) present *chindati* (Pkt. *chindai*). Similarly *lehī* is to be read at LV 197. 1 with v. l. for *lekhī*, and is to be connected with Pali *lehati* (blend of Skt. *lihati* and *leḍhi*).—From *dṛś* " see " we find aorists *addaśi*, *dṛśī*, *dṛśi*, *adṛśī* and °*śi* (sometimes with Prakritic *a* for *ṛ*; the augmented forms are written with either single or double *d*, but always pronounced with *-dd-*, as the meter proves). Here again we find, to be sure, no present **dṛśati*. But we must assume that the type started from the old thematic aorist *adṛśat*, which from the Prakrit standpoint would be felt as an imperfect to a thematic present stem *dṛśa-*. This clearly underlies Pali *addasaṃ*, *addasa* or °*sā* (Geiger, § 162. 3), which has modified it only by doubling the *d* in imitation of the type *addakkhi* (Skt. *adrākṣīt*). Our language has this same modification, and in addition has replaced the old thematic endings by those which had come to be felt as appropriate to the aorist. I find a confirmation of this theory in the 3 pl. form *adṛsu* (for *adṛśu*, pronounced *addṛśu*) LV 27. 22, for Skt. **adṛśuḥ* = *adṛśan*, cf. Pali *addasuṃ* (in Pali all 3 pl. aorist forms end in *uṃ*). See § 32, and for the further adaptation of the same original to *a*(*d*)*dṛśāsi* etc. see below, § 28.—In another place I shall shortly show that thematic present stems were used as bases for the whole verb inflection in the protocanonical Prakrit.

respects like actives): *vādyiṣu* "were made to sound" LV 194. 2 (*vādyate*, pass. to *vādayati*).—*dṛśyiṣu* "were seen" LV 74. 12 (*dṛśyate*).—*vādhyī* LV 222. 6 (to *bādhyate*, pass. of *bādh*).

§ 9. Particularly common are such forms from stems in *-aya-* (causative or denominative). A few examples: *deśayi* SP 57. 14, *prakāśayī* SP 23. 10, *nāmayī* LV 74. 2, *samayi* (for *śam°*) LV 366. 20. These *-aya-* bases sometimes drop the syllable *ya*, in aorists as well as in other forms (including presents): *snāpi* LV 271. 12 (aor. to *snāpayati*).

§ 10. But when this *aya* is Prakritically contracted to *e* (which occurs very often), it is no longer possible to form *this* type of aorist from the contracted stem in *e*; only the *s*-aorist type can be formed from a stem ending in a vowel (except the thematic *a* which is dropped). Hence *thapesī* from *sthapayati*, LV 169. 3, etc. (see below § 24).

§ 11. List of forms in *ī* or *i*. Except as stated above in note 3, they occur only in verses (but note *prābhañjīt* in prose, § 6, and plural forms, §§ 18, 20), and the ending short *i* is found only (or almost only) when the meter requires a short syllable; when the meter is indifferent or requires a long, *ī* is used. They are used for all persons and numbers, tho distinctive forms for other persons and numbers also occur, especially for the 3 pl. For the 1 sg. there is one doubtful occurrence of the ending *īṃ*, formed on the analogy of Skt. *īs*, *īt* (cf. Pali *iṃ*): *darśayīṃ* SP 90. 3 (but several mss. read *ī* or *i* without *ṃ*).

§ 12. Other 1 sg. cases: in SP, *prayojayī* 90. 5; *addaśi* 9. 8 (so emended in ed. for the metrically impossible *addarśi* of the mss., confirmed by *addaśi* 3 sg. LV 194. 12), *bhaṇi* 56. 6, *saṃkṣobhi* 56. 8, *deśayi* 57. 14. In LV, *praṇipati* 361. 19 (? or for *°pate*, 1 sg. pres. mid.).

§ 13. 2 sg. (no case noted in SP), in LV, *uddharī* 195. 12, *sampaśyī* 195. 20, *khedayī* 221. 8; and (with *i*) *labhi* 53. 4, 166. 14, *jani* 91. 17 (*mā khu jani viṣādu* "do not produce despair [become despondent]"), *tyaji* 165. 9, 16, 166. 18, 167. 2, 7, 168. 9, *paricari . . . druhi* 165. 18 (? perhaps gerunds), *upekṣi* 172. 18, *bhasi* 235. 9 (from *bhāṣ*, with *a* for *ā* m. c., and *s* for *ṣ* as very often), *khali* 362. 11 (v. l. *skhali*; "and thou hast not stumbled . . ."), *bhindi* 370. 22.

§ 14. 3 sg.: in SP, *avabhāsayī* 9. 4, *prakāśayī* 23. 10, 193. 4, *dhyāyī* 23. 12, *ārāgayi* (v. l. *ārādhayī*) 27. 12, 384. 6, *cintayī* 99. 3 (read with O), *paripācayī* 204. 13, *deśayī* 292. 12; and (with *i*) *ādhārayi* 25. 10, *darśayi* 64. 2, 193. 6, *cintayi* 87. 5, *ukrami* (for *o-krami*, from *ava-kram*) 95. 8, *saṃśodhayi* 114. 1, *aṇvi* 258. 10 (from Prakritic *aṇvati*; read *ananyakarmāṇvi daśaddiśāsu*; wrongly emended in WT). In Laṅkāvatāra Sūtra, *alapī* 23. 14. In LV, *nāmayī* 74. 2, *gacchī* 81. 11, *niṣkramī* 92. 12, *avacī* 109. 16, 165. 14, 199. 11, 304. 3, *jahī* 134. 17, *niścarī* 179. 5, 182. 19 (read *śabd' evarūpas tur°* *niś°*), (*a*)*vartayī* 220. 22, *ālapī* 221. 15, *nirīkṣī* 230. 4, *harṣī* 232. 16, *nirodhayī* 236. 5, *raṇī* 236. 10, *āruhī* 236. 15, *vivarī* 236. 17, *vrajī* 236. 18, *nipatī* 282. 3, *saṃpratigrāhī* 316. 8, *saṃprapaśyī* 341. 12, *osarī*[*r*] 357. 3 (read *°sahasrāṃś c' osarī*[*r*] *ātma°*; the form is 3 sg. and the final *r*, if it is to be adopted, must be considered a sandhi-consonant or "Hiatus-bridger"), *cańkramī* 368. 16, *kṣipī* 387. 7, *vikramī* 412. 1; and (with *i*) *cali* 92. 3, *muñci* 92. 20, *gacchi* 133. 13, 240. 21, 421. 13, *dhyāyi* 133. 21, *avaci* 135. 1, 329. 7, 370. 14, *prekṣi* 135. 8, 241. 11, *prajāni* 151. 15, *chini* 165. 22, *sravi* 166. 2, *bhaṇi* 166. 17, 20, 233. 7, *addaśi* 194. 12, 22, *adṛśi* (pron. *add°*) 194. 15, 197. 15, 17, *dṛśi* 230. 4, *kari* 200. 9, *codayi* 220. 1, *nirmiṇi* 219. 19, *avaśiri* 230. 12, *pratibhaṇi* 231. 21, 232. 3, 10, *upagami* 243. 12, *snāpi* 271. 12, *vraji* 299. 11, *abhivarṣi* 301. 6, *samayi* (*śam°*) 366. 20, *paribhuñji* 387. 7, *vyākari* 393. 12.—In LV 299. 12 *anubuddhi* (no v. l.) is certainly an aorist connected with the root *budh*; similarly *buddhiṣū* LV 220. 12 (§ 18 below), all mss. also *-ddh-*. With these are to be compared the gerunds *buddhitvā* LV 163. 19, 164. 12, *buddhitva* LV 231. 19, 355. 15; the pple. *buddhita-* (so mss., emended by Senart m. c. to *budhita-*, and doubtless so pronounced) Mhv. i. 209. 23, and—most important—the present optative *buddhema* LV 361. 10 (with acc. *sarvajñatāṃ*; so at least Lefmann reads with best mss.; v. l. of inferior mss. with Calc. *budhyema*). The aorists and the gerunds and pple. all seem to imply a present *buddhati*, the stem of which actually occurs in *buddhema*. At first I inclined to emend all these forms to *budhy-* instead of *buddh-*, connecting them with the stem of *budhyate*; cf. the future *budhyiṣyate* SP 343. 14 (where however two mss. read *buddhisyate*, so, with dental *s*). I am still not sure that this is not the true solution, the *buddh-* forms being either corruptions or orthographic or phonetic variants of *budhy-* forms. Since however the *-ddh-* forms occur rather persistently, I am now inclined to

accept a present *buddhati*, a sort of denominative to the stem *buddha-*. Whether this or the regular form *budhya-te* be accepted as basis, in either case we have aorists (and gerunds, futures, participles) based on a thematic stem.

§ 15. 1 pl.: *praṇipati* LV 363. 10 (? Foucaux as 3 pl.; cf. parallel *vande* in next two verses). I have noticed no 2 pl. case.

§ 16. 3 pl.: in SP, *vivardhayī* 131. 1, *niṣpādayī* 131. 2, *upasaṃkramī* 190. 10, and (with *i*) *darśayi* 49. 7, *tārayi* 51. 8, *śruṇi* 90. 4, *kṣipi* 94. 12, *abhyayāci* 191. 8, *abhāṣi* 194. 3, *pravarṣi* 331. 1, *āropayi* 331. 11 (misprinted); in LV, *kṣipī* 92. 16, 338. 21, *visnapī* 93. 2, *anvākramī* 94. 17, *niścarī* 175. 16, 182. 21, 183. 2, 4, 6, 8, 10, 11, *patī* 194. 10, *lehī* 197. 1 (so with v. l., for ed. *lekhī*, to **lehati*, see note 4), *vādhyī* 222. 6, *nyasī* 236. 14, *pravarṣī* 285. 6, *abhihanī* 329. 4, *onamī* (*ava-nam*) 356. 19, *pṛcchī* 421. 18; and (with *i*) *upagami* 49. 7, 50. 11, *vidarśi* 49. 10, *paśyi* 75. 15, *viheṭhi* and *vihiṃsi* 75. 16, *vivarjayi niveśayi* 158. 13, *chādayi* 158. 19, *anuravi* (so, for *tatu ravi*) 164. 22, *muñci* 236. 22, *avaci* 241. 1, 329. 21, *abhijāni* 342. 3 (*jānati* = *jānāti*), *ābhāsi* 342. 4, *adhyeṣi* 416. 3.

§ 17. *Distinctive 3 plural forms.* Tho *ī*(*i*) is often used with 3 pl. subjects, we find also quite commonly Prakritic 3 pl. forms related to the Skt. ending *iṣuḥ*. They are formed, like the *ī*(*i*) forms, from present bases in thematic *a* which is dropped. In AMg. the ending appears in the form *iṃsu*; in Pali both in this form and as *isuṃ*. In the Prakrit which underlies our dialect the commonest ending was probably *isu*, the direct phonetic heir of Skt. *iṣuḥ*. Here it appears normally as *iṣu*, rarely as *isu* (our mss. very often write *s* for *ṣ* in other forms, so that the variation in sibilants is probably of no specific significance). It is thus different from Pali in never showing final anusvāra. On the contrary, when the meter requires a long final syllable, neither the Pali form nor the Skt. is used, but (regularly) the *u* is lengthened to *ū*. When the meter requires a long penult, the vowel *i* may be lengthened to *ī*. But more often the alternative ending is used which corresponds to the exclusive AMg. and alternative Pali ending *iṃsu*. This appears in our dialect, at least in the mss., in a remarkable variety of spellings. Aside from the normal lengthening of final *u* (when metrically required), we find also, tho rarely, the Skt. ending *uḥ* in its various sandhi forms. The sibilant before it is almost always written *s*, rarely *ṣ*, contrasting remarkably with the alternative ending which

is normally written *iṣu*, rarely *isu*. Instead of the anusvāra before the sibilant we find the consonant *n* quite frequently, and sometimes—if we may trust the editors' statements—even *t*. And finally, for the penultimate vowel *i* we sometimes find *e* written. Thus, altogether, we find the following array of endings: *iṣu, iṣū, isu, īsu, īṣū* (all to be regarded as variants of one form of the ending, corresponding to Pali *isuṃ*); and *iṃsu, iṃsū, iṃṣū, insu, insuḥ* (°*suś ca*), *itsu, ensu, eṃsu* (all variants of the alternative ending corresponding to Pali and AMg. *iṃsu*). We shall treat these two groups separately.

§ 18. 1. *iṣu* and congeners. The fundamental form is clearly *iṣu*; the others are variants, chiefly m. c. (on the writing *s* for *ṣ* see above). Almost limited to verses. In the prose of LV has been noted only (397. 21-2) *antaradhāyiṣu* (so probably to be read with the best ms.) or °*ṣuḥ* (so Lefmann with other mss.), "disappeared". This would in Skt. be a passive, *antar-adhīyanta*; our form is to be connected with the Pali present *antara-dhāyati*, and what we have is probably a Prakritic *antara-dhāyiṣu* (so, rather than *antar-adhāyiṣu*). In the prose text of SP as printed I have noted no case, but the Kashgar mss. sometimes show such forms (examples above, § 5). The rest are all in verses. The vowels *i* and *u* are always short in metrically indifferent positions.

isu: *paśyisu* SP 194. 7 (no v. l.), *snapayisu* LV 234. 4 (the only v. l. is °*yiṃsu* which is unmetrical).

iṣu: in SP, *kurviṣu* 10. 6, *paśyiṣu* 15. 5, 325. 6, *krīḍiṣu* 50. 12, *krandiṣu* 84. 12. In LV, *hiṃsiṣu* 28. 6, *gacchiṣu* 74. 6, *dṛśyiṣu* 74. 12, *vādiṣu* 75. 13, *rohiṣu* 75. 20, *nandiṣu* 76. 2, *namiṣu* 92. 8, 158. 7, *udīkṣiṣu* 114. 18, *abhivandiṣu* 116. 4, *prabhaṇiṣu* 164. 20, *raviṣu* 167. 12 (so, or *ravisu*, with mss.), *pratibodhayiṣu* 173. 9, *parivartiṣu* 173. 16, *muñciṣu* 193. 17, *vādyiṣu* 194. 2, *varṣiṣu* 222. 7, *prekṣiṣu* 240. 18, *prapatiṣu* 329. 13, *staviṣu* 329. 14, *nirīkṣiṣu* 340. 8, *bhāṣiṣu* 352. 20, *pūriṣu* 387. 3, *udāhariṣu* 413. 21, *adhyeṣiṣu* 414. 10 (so for ed. °*tu*).

iṣū: in SP, *parihāyiṣū* (*-hāyate*) 170. 2, *adhyeṣiṣū* 190. 11, 191. 3, *paśyiṣū* 323. 14. In LV, *gacchiṣū* 74. 10, *virociṣū* 122. 22, *vrajiṣū* 169. 12, *muñciṣū* (must be read m. c. for °*ṣu*) 217. 1, *buddhiṣū* (see § 14, end) 220. 12, *raṇiṣū* 222. 7, *abhivandiṣū* 236. 12, *bādhiṣū* (must be read m. c. for °*ṣu*) 357. 6.

īsu: *vartīsu* LV 402. 14. (In *udgṛhṇīṣu*, v. l. of O in SP 181. 9, prose, the *ī* is to be explained—if the form be accepted—as a remembrance of the Skt. present stem °*gṛhṇī*°.)

īṣū: *kārāpayīṣū* SP 50. 16 (no v. l.) and 51. 1 (v. l. of O °*yinsu*).

iṣus: the normal Skt. ending, in a form otherwise Prakritic, when the meter requires a long ultima, has been noticed only once: *kṣipiṣus* LV 282. 16. Based on the present *kṣipati*, like the other forms of this section.

§ 19. 2. Much rarer are the forms of the alternative ending, with nasalized penultimate vowel, corresponding to AMg. and (alternatively) Pali *iṃsu*. But the variations in spelling are perhaps even more numerous and curious. The lengthening of final *u* m. c. calls for no comment. The sibilant is, this time, almost invariably written *s*, a clear indication of direct inheritance from a Prakrit dialect. The preceding nasal seems to be written *n* almost as often as *ṃ*; but this may be merely a matter of orthography. At least similar variations occur in other forms, outside of this ending; there is a great deal of confusion in the mss. between *n* (and *m*) and *ṃ*, both internal and final. Further, even *t* is written for *n* (or *ṃ*). On this see Senart Mhv. 1. xvi, who thinks of the Vedic insertion of *t* between (final) *n* and (initial) *s*, but also suggests that *t* for *n* in our texts may be largely or wholly a matter of graphic confusion, due to the similarity of the signs for *n* and *t* before consonants. If this last is the true explanation, there would be no real linguistic importance in the matter. Our texts, apparently, present *t* for *n* very rarely. But the mss. of the Mahāvastu, contrariwise, are stated by Senart to read *t* very commonly, even regularly, for *n* in these 3 pl. forms; nevertheless Senart himself in his edition prints uniformly *n*. Puzzling also is the writing (extremely rare in our texts but very common in Mhv., see Senart l. c.) of *e* for the penultimate *i*. Senart refers to "Pali and Prakrit forms in *eṃsu* beside *iṃsu*". I find no record of such forms in Pali; and Pischel § 516 considers the true AMg. ending *iṃsu*, tho he records the occasional spelling *eṃsu*, regarding it merely as a Prakritic writing for *iṃsu* (cf. his § 119). Aside from *sādheṃsu* LV 387. 1 (which in the first place is an emendation, tho I think a plausible one, and in the second place belongs to *sādhayati*, Pali *sādheti*, and may have its *e* by contamination from such present forms), I have found in SP and LV only the isolated *śruṇensu* SP 52. 12 (with no v. l.); cf. Mhv. 1. 42.

12 *śruṇensuḥ* (mss. all °*tsuḥ*), and many similar forms in Mhv. Three explanations of the *e* for *i* seem to me conceivable: (1) that given by Pischel, above, for AMg. *eṃsu* = *iṃsu*; (2) influence of stems in Prakritic *e* from *aya*, cf. *sādheṃsu* just quoted, and cf. further the great extension of such stems in *e* in Prakrit generally (Pischel § 472); (3) influence of the 3 sg. forms in *e* (originally optative?) which seem undoubtedly to be used as aorists, see § 32 below. Of these explanations the first or the second will be most apt to be right.

§ 20. No forms of this type occur in the prose of the editions as printed. But for *parāhananti* SP 69. 11 (prose), O reads °*hanitsu* (for °*haninsu*, see above); for *abhāṣetām* SP 267. 2 (prose), LaVP 1074 has *bhāṣinsuḥ*; and several such readings occur in the Hoernle fragments (Lüders 152 *upasaṃkraminsu* for SP 270. 11 and *nadinsu* for SP 271. 2; Thomas 134 *pravartayinsu* for SP 327. 11, and 136 *prādurbhavinsu* for SP 329. 2, *prapatinsu* for SP 329. 3). I have little doubt that these semi-Prakritic forms are all closer to the original readings than the correct Skt. forms of the Nepalese mss. adopted in the editions.—The final *ḥ* (or equivalent) of Skt. is preserved (as v. l., that is in some mss.) in the prose *bhāṣinsuḥ* of LaVP above, and in *niryādayinsuś ca*, v. l. of O in SP 191. 2 (verse).—The remaining occurrences are all in verses.

iṃṣū: *raṇiṃṣū* LV 233. 18. All mss. have *ṣ* (Calc. *raṇiṣu*, which is unmetrical). This is the only case I have noted containing *ṣ* rather than *s*; note that in the same line occurs *patiṃsū* with the usual *s*.

iṃsu: *abhistaviṃsu* SP 191. 3; *avaciṃsu* SP 192. 10 (O °*nsu*, one Nepalese ms. °*tsu*); *śrāvayiṃsu* SP 194. 5; *nyasyiṃsu*, probably to be read at LV 222. 3 (from *ny-asyati*, "cast down"; the mss. vary greatly; Lefmann *nyasayiṃsu*, which would also be possible metrically); *ābhāṣayiṃsu* LV 353. 6; *stavayiṃsu* LV 414. 10 (Calc. °*yitsu*).

iṃsū: *saṃśrāvayiṃsū* (ed. °*su*, unmetrical; O °*nsu*) SP 194. 2; *kṣipiṃsū . . . bhramayiṃsū* LV 81. 7; *patiṃsū* LV 233. 18; *staviṃsū* LV 233. 19.

insu (for prose cases see above): in SP, *darśinsu* 55. 12 (some Nepalese mss. °*śensu*, others °*śansu*); in 50. 3 ff. read probably *karinsu* (several times) with O; O also has v. l. *kārāpayinsu* 51. 1, *ālikhinsu* 51. 6. In LV, *snapinsu* 93. 21 and 22 (for the

latter all mss. except the best °*itsu*), *upagaminsu* 94. 21 (v. l. °*itsu*).

insū?: in SP 51. 8 O has v. l. *samādapinsu*, which is unmetrical (°*sū* would be required), for ed. *samādapentā*.

eṃsu and *ensu*, see above, § 19, end.

§ 21. Other (non-Sanskritic) *iṣ*-aorist forms. There remain to be mentioned a few stray forms, most of which are not particularly abnormal in formation but are not recorded in Sanskrit. Thus, *onamiṣṭa* LV 295. 7 would be (but for the Prakritic phonology of the preverb) a regular 3 sg. middle *iṣ*-aorist from *ava-nam*; but the root *nam* is not authorized to form an *iṣ*-aorist in Sanskrit. And *vilambiṣṭhāḥ* LV 217. 11 (prose) would likewise be a regular 2 sg. middle; in Skt. *alambiṣṭa* is quoted from grammarians but according to Whitney's *Roots* not recorded in literature. While *staviṣṭa* LV 298. 6 is regular Sanskrit in that form (but for lack of augment and the fact that the subject is 3 pl.); but (tho no v. l. is recorded) the meter requires *stāviṣṭ'* (the final *a* must be elided before the following initial *a*-), with vṛddhi root-vowel, a transfer from the vocalism of the active (or lengthening m. c.?). More anomalous is *nyaṣīt* LV 271. 19, apparently "deposited, provided" (*bhadrāsanaṃ*; Foucaux apprêta), from *ni+as* "throw." This root has no *iṣ*-aor. active in Skt.; if one existed it would be **āsīt* (homonymous with *āsīt* "was"); the grammarians quote a middle *āsiṣṭa*. The *ṣ* for *s* is anomalous, but apparently found in all Lefmann's mss.; only Calc. has *nyasīt*. I have noticed no other case of *ṣ* for regular *s*, tho *s* for *ṣ* is frequent. Even *ṣ* for *ś* is rare. Accordingly I am inclined to regard *ṣ* here not as a phonetic confusion of sibilants but as an analogic change, due to the influence of *s*-aorist forms in *ṣīt* such as *aśrauṣīt*, *anaiṣīt*, in which the sibilant belongs to the aorist suffix, not to the root as here, and follows a non-*a* vowel. The short *a* is of course also anomalous but may be regarded as m. c.

§ 22. The inherited Skt. imperfect *āsīs*, *āsīt*, "was", is treated in our language like an *iṣ*-aorist, as we might expect (in fact it was originally formed in imitation of this type). It may appear as *āsī*, as in LV 168. 15, 169. 1, 9 (in all three most mss. *āsīd* [or with *ś* for *s*], tho with 2 sg. subject), or, when the meter requires a short, as *āsi* (SP 27. 6, LV 76. 1, 194. 3; with 2 sg. subject SP 312. 15 [? here perhaps *asi*, pres., is intended], LV 167. 21; with 1 sg. subject SP 62. 13).

II. Descendants of Sanskrit *s*-aorists, and similar forms

§ 23. Geiger § 167 (cf. 163, 165. 2) noticed that Pali, which freely forms aorists of the *iṣ*-type from present stems in general (he should have said, I think, from thematic presents), does not form them from stems in final long vowel, but in such cases has forms which look like Skt. *s*-aorists. Our language seems to follow the same principle in general; and Pischel § 516 contains material enough to suggest that it prevailed in AMg. too, tho he does not formulate it. It would hardly be going too far to say that in all three, the general rule is to form *s*-aorists from bases (not only present stems, in fact more often "roots" in our dialect) ending in vowels (or at least in long vowels), while otherwise, and particularly from thematic presents, *iṣ*-aorists are made (after dropping the thematic vowel).

§ 24. We saw above that when *aya* of causative and denominative stems is contracted to *e*, *s*-aorists are made from such stems. So *thapesī* LV 169. 3 (with dental *s*, perhaps—like loss of initial *s*—pointing directly to a Prakrit form, but *s* for both *ṣ* and *ś* is very frequent), from *sthapayati*; and *kīrteṣy* SP 63. 9 (with cerebral *ṣ*; read with WT *kīrteṣy atītān*). From the Prakritic standpoint, these forms fall into a common category with *parigraheṣīd* SP 204. 8, cf. Pali *aggahesi*, altho historically the *e* here is doubtless an inheritance from Skt. *ai*, cf. *agrahaiṣam* AB. etc. Pali has aorists in *esi* of still different historic origins (Geiger § 165). And AMg. has, besides forms from *aya* verbs like *kahesi* (: *kathayati*), also *annesi* (**ajñāyiṣīt*) from *jñā*, *ahesi* (**abhaviṣīt*) from *bhū* (note that the BSkt. aorist of *bhū* is different, see below), which historically stem from *iṣ*-aorists but by Prakrit phonetic changes have fallen into this group; and perhaps others.

§ 25. Another *s*-aorist form, after a vowel, is *aśroṣīt*, twice in the prose of LV (143. 13, 300. 11-12), a slightly Prakritized equivalent of Skt. *aśrauṣīt*. Note further *samavāsthāsīt*, v. l. of O (probably to be adopted) for ed. *samavātiṣṭhat* SP 239. 3 (prose); *adāsīt* SP 250. 2 (prose; no v. l.); and in a verse *asthāsi* SP 86. 4. And finally, the very common *abhūṣi*, which points to a protocanonical Prakrit aorist of *bhū* different from any found in either Pali or AMg., which (aside from non-sibilant aorists) show only *ahosi* (also *ahesuṃ*) and *ahesi* respectively. If the protocanonical Pkt. had

agreed with them, we should find **abhoṣīt* or **abheṣīt* (or °*ṣi*, °*ṣī*), which have not been found. This is the more surprising because we should expect parallelism with the future, which in BSkt. actually does appear as *bheṣyati* (regularly) or *bhoṣyati* (rarely). Another surprising thing about this form is that, while in prose the form *abhūṣīt* occurs (at least in the Kashgar recension of SP), *abhūṣī* seems not clearly to be found except in verses and there only in positions where meter requires a long final (see note 3 above), and the regular form seems to have been *abhūṣi* with short *i*. This occurs in metrically indifferent positions SP 27. 4, 93. 3, 192. 5; and according to the Lüders fragment, Hoernle 150 infra, once even in prose (this word is omitted altogether in the edition of SP). Other occurrences in verses: SP 26. 13, 56. 11, 12, 57. 5, 157. 11, 204. 11, 384. 2; with 3 pl. subjects, 49. 14, 50. 1, 2, 6, 12, 14, 16, 51. 2, 4, 6, 14, 52. 3, 12, 190. 15. In LV it is much rarer: 27. 19, 123. 8 (read *abhūṣi* with best ms. A for *subhū*°), 163. 16, 415. 6.

§ 26. After consonants, *s*-aorists are rare except when they preserve normal Skt. forms. In SP 468. 7 (prose), a late passage, the edition reads *akṣaipsīt* (from *kṣip*), a form quoted by Hindu grammarians but not known in literature (Whitney, *Roots*); but the reading is very uncertain; only three Nepalese mss. have the word at all, and of these only one reads *akṣaipsīt* (the others *akṣe*°, *akṣī*°), while O (the Kashgar recension) has *kṣipataḥ*, a form which I do not understand. Note further *pravekṣī* LV 136. 6 (in a verse); *viś* has no *s*-aor. in Skt.; if it existed it should be **avaikṣīt*.—LV 364. 11 has a 3 pl. aorist *dikṣiṣu*, from *diś*. Formally it could be described as a "*siṣ*" aorist (Whitney's 6th class). But its nearest relative among recorded forms is the (7th or *sa*-) aorist *adikṣat*, of which the 3 pl. *adikṣan* has here been furnished with the Prakritic 3 pl. ending customary in our dialect.—Yet more anomalous is *lapsi* SP 190. 2, aorist to *labh*. Whitney, *Roots*, mentions "*alabdha*, *alapsata* B.; *lapsīya* S."; *lapsi* might pass for a modification of this type by giving it the normal Prakritic ending. One is almost tempted to assume contamination from the desiderative (*lipsate*, *lipsati*).

§ 27. Apparent exceptions, showing *s*-aorists after a consonant, are further such forms as *vyākarṣu* "they predicted" SP 27. 3; *vyākarṣīt*, probably the true reading underlying the corrupt *vyākarṣitaś* in LV 39. 19 (prose), and *harṣur* "they removed" LV 271. 16.

In both Pali (Geiger § 163. 4) and AMg. (Pischel § 516) such roots (in original *ṛ*) show aorists *akāsi, ahāsi,* which conform to our rule, since they resemble roots in *ā,* tho of course historically derived from Skt. *akārṣīt, ahārṣīt.* It might be suggested that our forms are direct inheritances from those Skt. forms, with *ā* shortened to *a* by Prakrit phonology before a consonant cluster. But, rather surprisingly, I have not found evidence for such shortening in our dialect, at least in the orthography of our mss. It is more likely that *vyākarṣīt* is an imperfect Sanskritization of **-kāsi* of protocanonical Pkt., influenced by other forms in *kar-.* Cf. such aorist forms as *vyākari* LV 393. 12, *kari* LV 200. 9, *udāhariṣu* LV 413. 21, based on the thematic presents *karati* and *harati* (the former is found e. g. in *kareya* LV 335. 19, *satkareta* SP 228. 14).

§ 28. The blend-form *(a)dṛśāsi,* etc. We find also (formally in accord with our rule) certain complicated and interesting blend-forms from the root *dṛś* which agree closely with Pali forms, and (presumably like them) must stem from the protocanonical Prakrit. They are ultimately based on the old thematic aorist *adṛśat*; see Geiger §§ 162. 3, 165. 1, and our note 4 above. When augmented, the forms, tho usually written with single *d,* are always pronounced *add-* as the meter proves. On the basis of Pali *addasā* is formed Pali *addasāsi,* 3 sg., with *s*-aorist termination; Geiger § 165. 1 correctly explains the form as based on the analogy of *akā* : *akāsi, adā* : *adāsi,* all aorists (from *kṛ* and *dā*). It appears in our dialect, half-Sanskritized, as (3 sg.) *adṛśāsi* (pronounced *add°*) LV 196. 12, 15, 19. In the same context the same form occurs as 2 sg., LV 195. 11, 15, 19. Historically it can be justified as both 2 and 3 sg. (Skt. *°sīs* as well as *°sīt*). Since however the form in *-si* looks like a 2 sg. present, we also find an analogical 3 sg. in *-ti* : *adṛśāti* (pron. *add°*) LV 75. 6, 194. 20, 197. 9, 241. 10. Very similarly Pali shows a 1 sg. *addasāmi* (Geiger § 162. 3). It is likely that in Pali, as in our dialect, these are direct inheritances from the protocanonical Prakrit, that is not native to the Pali dialect. Perhaps the blend forms implying interpretations as presents, with primary endings, may have been assisted by the fact that there was no present from this root in Sanskrit or Pali, or presumably in the protocanonical Prakrit (forms of *paś* being substituted); and since the augment was often if not usually lost, the association of such forms with past time must have been weak. In other words,

the blend forms with "primary" endings may have been felt as presents, not preterites.

§ 29. Finally, in SP 51. 6 occurs a strange form *kṛtāsi* (it may also be understood as *akṛtāsi*), which, if the true reading, can hardly be anything but a 3 sg. aorist (with 3 pl. subject) to *kṛ*. The edition with all Nepalese mss. reads *nakhena kāṣṭhena kṛtāsi vigrahāṃs*, "(who) with fingernail or stick have made images (of the Buddha)." The Kashgar mss. have a quite different reading, containing the verb-form *ālikhinsu*, from *ā-likh*; this may possibly be the true reading. WT emend to *kṛtāvi* (= °*vinaḥ*, nom. pl. to *kṛtāvin*, as periphrastic perfect); but on paleographic grounds the emendation is not attractive. Perhaps (*a*)*kṛtāsi* may be a blend-form like *adṛśāsi*, based on *akṛta*, 3 sg. aor. middle. Pali makes frequent use of such 3 sg. middles, ending in *tha* (with aspirate instead of Skt. *ta*, cf. Geiger § 129); e. g. *ajāyatha* etc., Geiger § 161. In view of these forms and Pali *akā* (3 sg. aor., cf. Vedic *akar*), it is not hard to suppose a Pkt. **akata* (or **akatha*), from which **akatāsi* would be produced just like Pali *addasāsi* to *addasā*; this would then be half-Sanskritized in our dialect as (*a*)*kṛtāsi*.

III. Prakritic descendants of the root aorist

§ 30. Here we need note only forms of the root *bhū*. The 3 sg. *abhūt* may be used as such for other persons and numbers, as regularly with all 3 sg. forms in our dialect. So even in prose, (*a*)*bhūt* as 1 sg. SP 22. 11, LV 239. 18, and as 2 sg. SP 64. 11. And in verses, as 3 pl., SP 46. 5, LV 74. 15, 138. 3, 164. 1. By Prakritic phonology the *t* may be dropped, leaving *abhū*; and that even before a following initial vowel, leaving hiatus, which would have been avoided by keeping the Sanskrit form, SP 157. 10. The same (*a*)*bhū* is used as 2 sg. (here perhaps directly representing Skt. *abhūs*) LV 165. 13, 17, 21; 166. 3, 7, 11, 15; 170. 19; and as 1 pl. LV 252. 11 ("we [gods] were invited by you of old to the Sacrifice of the Good Law"; misunderstood by Foucaux). Furthermore, the final *ū* may be shortened m. c., yielding *abhu*: SP 25. 10, 204. 9, LV 237. 14.

§ 31. Once in the prose of LV (83. 13) occurs a 3 dual *abhū-vatāṃ*, for *abhūtāṃ*, with insertion of thematic *a*. This may be taken as a kind of blend between the regular root-aorist and the

thematic aorist (according to Geiger, imperfect) *abhuvat* (in Skt. only Vedic) = Pali *ahuvā* (Geiger § 162. 2). Weller (*Ueber die Prosa des Lalita Vistara* 52) proposes to emend the text to *abhuvatāṃ*, bringing it into direct agreement with such Pali forms. But this is hardly necessary. Weller exaggerates when he quotes Senart (Mhv. I. xii) as saying that there is "no difference" between the writing of *ū* and *u* in Nepalese mss.; what Senart says is rather that it is "very often impossible to distinguish with certainty" between *ū* and *u*. It is however true that LV 222. 11 and 21 (both verses) show a form *bhuv'* (*imu*; before initial *i*), a 3 sg. preterite which might be directly equated with Pali *(a)huvā*, Vedic *abhuvat*. Cf. also AMg. *bhuvi(ṃ)*, Pischel § 516; *bhuv'* in our dialect could stand equally well for *(a)bhuva(t)* or *(a)bhuvi* (= **abhuvīt*; note the following *i*-!).

IV. Prakritic descendants of imperfect and *a*-aorist

§ 32. In Pali the imperfect has fused with the aorist, particularly the thematic aorist (Geiger § 159, II, IV), and all preterite 3 pl. forms end in *uṃ*, which of course corresponds to Skt. *uḥ*. In our language we find a few 3 pl. preterite forms in *uḥ*, from thematic stems, which deserve mention in this connection. In footnote 4 above, mention was made of *adṛśu* (m. c. for °*śuḥ*; written with *s* for *ś* as often) LV 27. 22, corresponding to Pali *addasuṃ*, Skt. *adṛśan*. Another clear case is *vimṛṣuḥ* LV 385. 19, for *vimṛśuḥ*, 3 pl. preterite to *vimṛśati* (Skt. *vy-amṛśan*). In the light of these forms I have little hesitation in interpreting thus (rather than as optative 3 pl., for *deśayeyuḥ*) the form *deśayuḥ*, which occurs once or twice in textually rather uncertain passages. In SP 57. 16 the edition reads so, probably rightly; O °*yikhu*, intending °*yiṣu*, which is impossible metrically but confirms the interpretation as preterite 3 pl. In SP 272. 10 the ed. also has *deśayuḥ* without report of v. l.; see § 36 for the Kashgar reading *deśayī*, which suggests that if *deśayuḥ* was the true reading it belonged formally to the aorists, even if optative (or future?) meaning must be attributed to it.

V. Blends of aorist and optative forms (also with future meaning)

§ 33. Pischel § 466, end, presents convincing evidence to show that in AMg., "so unerklärlich es scheint," forms which can hardly be other than optatives are used in the sense of past indicatives.

Among these he mentions 3 sg. forms in *e* (Skt. *et*). One such form has turned up in our texts: *spṛśe sa bodhiṃ* SP 190. 7, "he attained enlightenment." The meaning is certain; it occurs in the description of how a past Buddha attained his goal. Some equally clear cases occur in the Mahāvastu, e. g. iii. 53. 16 (prose) *prajñāpayed aham . . . niṣīde bhagavāṃ*, "I offered . . . the Lord sat down." Here *niṣīde* (better read °*det*, see critical note) is 3 sg., and *prajñāpayed* 3 sg. used as 1 sg. (unless the *-d* be considered a hiatus-bridger); simple narrative preterites are meant, despite the apparently optative forms.

§ 34. Not recorded in Prakrit or Pali, so far as I can see, is the reverse phenomenon, which is commoner in SP and LV, namely the use of forms in *ī* (also *i*, m. c., which however can usually be taken as a normal optative with *i* for *e*) which look like 3 sg. aorists, but have the meaning of optatives (either jussive or potential), or even apparently of simple futures. Since the optative approaches very closely the meaning of the future at times, all these may perhaps be classed as "optative" in meaning. We may recall that both *e* (opt.) and *ī* (aor.), from Skt. *et* and *īt*, are often reduced to short *i* in our dialect, almost always to be sure when the meter requires a short. Does this fact lie at the root of this confusion and that mentioned in the last paragraph? At all events it seems impossible to deny the facts as stated.

§ 35. Thus the following forms in *ī* (once *īt*) cannot have past meaning, and appear to be optatives in force:

nābhāsī LV 122. 16 "would not shine" (a parallel *bhavet* occurs in the same stanza).

nābhāsī na tapī LV 122. 19 (also closely attended by parallel *bhavet*).

praviśī bhavanta(ḥ) LV 115. 4 "let your worships enter."

sarveṣu maitrībala so hi darśayī SP 291. 12 "he shall show power of kindness to all (beings)." An injunction.

vyākarī LV 57. 3 "(brahmans who) shall (or might, may, could?) explain (my dream)"; Māyā speaks to the king.

niścarī LV 177. 9, and *dṛśī* 177. 12, "would (ever) depart . . . see;" apparently optatives of generalized statement.

na śraddadhī mahyam imāṃ vibhūṣitāṃ pitā mamāyaṃ ti na cāpi śraddadhīt SP 113. 10, "he would (will?) not believe in

this magnificence (so Kern; Tibetan *ḥbyor-ba* [read *-pa*?] 'wealth') of mine, nor yet would he believe, 'this man is my father.'" In the parable of the Prodigal Son. Since the father has not yet declared himself and the son has no reason yet to think of him as such, surely a past cannot be meant.

§ 36. The last two examples approach the borderline of the future. In the following the most natural interpretation would be as simple futures:

uddharī LV 195. 12, 196. 2, and *saṃpaśyī* 195. 20, in a passage containing predictions. Parallel to the latter (*māṃ saṃpaśyi* "will see me") is *māṃ draṣṭāsī* 195. 18 (periphrastic future, with *ī* for *i* m. c.), and all the parallel verbs are either futures or presents with future meaning. Cf. also *vyaktaṃ kumāra abhiniṣkrami* (short *i* m. c.) *bodhihetoḥ* LV 135. 14, "the prince is clearly going to go forth in search of enlightenment" (a definite prediction; if °*krami* stands for the optative °*krame*[*t*], the opt. has certainly merged pretty completely into the future in meaning).

The verses of SP xii (2-21), SP 271 ff., contain promises and predictions. Definitely future verbs are the rule, with occasional present forms in future meaning. But precisely parallel with them are *deśayī* 272. 5, and according to LaVP 1076 also 272. 10 (where the edition has *deśayuḥ*); *cārayī* 273. 3; according to LaVP l. c. *prakāśayīt* in 273. 10 for edition °*ye*.

Again in another passage SP p. 295 we find *saṃprakāśayī* line 2, *abhiniṣkramī* and *upasaṃkramī* 4, *pravartayī* 7, *deśayī* 8. Here most of the parallel verbs are present in form, and might possibly be interpreted as generalized statements (a meaning which would go with the optative). But *anuprāpsyate*, line 6, is future, and suggests that the whole passage is rather concerned with definite prophecy, which is really more appropriate. If these forms are "optatives", the optative meaning is stretched a good deal. But still less can they be considered preterites; yet formally they look like aorists.

THE ROOT FORMS sì(m) AND su_{11}(m), "TO GIVE," IN SUMERIAN

A. POEBEL
UNIVERSITY OF CHICAGO

IN A FOOTNOTE added to his review[1] of my publication *The Sumerian Prefix Forms e- and i- in the Time of the Earlier Princes of Lagaš*,[2] Thureau-Dangin remarks that the reading of the Sumerian verb form i-sì, "he has given," as i-sì(m) (of the root sim, "to give") "n'est fondée que sur S[b] dont le témoignage a été de tout temps sujet à caution en raison du fréquent complément -mu. La véritable lecture (sum) est maintenant donnée par le Vocabulaire de Chicago. Il faut lire ì-sum et distinguer nettement sì, sì(g) = *nadû* de sum, sùn (dialect zem) = *nadânu*."[3] Thureau-Dangin thus makes the following assertions:

(1). S[b], No. 2 (= CT 11, 24 ff.), col. 3, l. 7, is wrong in attributing the reading si-i to sì = *nadânu*.

(2). The real pronunciation of sì = *nadânu* is given by Chicago Syllabary, l. 123, namely, as sum, with a variant pronunciation sun as proved by the sign name *sunnu*.

(3). The reading of sì = *nadânu* as sum instead of sì is proved

[1] Published in AJSL 49 (1932-33), 269 f. The article here published was written four years ago (1932) immediately upon receiving Thureau-Dangin's review.

[2] Published as No. 3 of the Chicago Oriental Institute series "Assyriological Studies."

[3] Thureau-Dangin begins the note with the words: "Poebel donne p. 2 un tableau des formes de préfixe e ou i, en ne retenant que celles 'in which the vowel of the root is undoubtedly an *a*, *e*, *i*, or *u*, leaving aside for the moment all those cases in which either the root itself or the vowel is in doubt.' Ou, parmi ces formes figure ì-sì(m) où la voyelle est au premier chef incertaine." I wish to point out that, as the context clearly shows, the "uncertainty" debarred by the condition mentioned above refers only to those verbal roots whose vowel is not definitely either the open *a*, which causes the prefix to appear as e-, or one of the close vowels *i*, *u*, *e*, which cause the prefix to apppear as i-. The form i-sì(m), however, whether its root is to be pronounced sì(m) (which I think was the preferred pronunciation at the time of the early princes of Lagaš) or sum (which Thureau-Dangin recognizes as the only correct pronunciation), is rightly listed in the second group; for in either case the root contains a close vowel.

by the fact that sì is frequently followed by the so-called "phonetic complement" -mu.

(4). The *m* of the root form sum, as well as the *n* of the root form sun, "to give," is inamissible.[4]

(5). The reading sì (written si-i in the glossing of the syllabary) represents exclusively the first part of the root or roots sig, which Thureau-Dangin illustrates by the verb sì, sì(g) (i. e., the verb sig with dropped or preserved final *g*) = *nadû*, "to throw."

From Thureau-Dangin's remark it might seem that my own opinion is that the pronunciation of the root sì = *nadânu* was exclusively si(m). A glance at the alphabetic list of verbal forms at the end of the study on the prefix forms, however, will show that this is not the case, for on page 40 of that study the Sumerian verb for "to give" is registered not only in the form sì(m), but also in the form su_{11}(m), the latter form being based, of course, on Chicago Syllabary 127, that is, the very passage referred to by Thureau-Dangin in his review. Note, moreover, my remarks in GSG (1923), § 18, where I left it an open question as to which of the roots sì(m) and sum—both attested by syllabaries—is the more original.

As a matter of fact, my position is this, that both pronunciations existed, although it is my belief that sì(m) represents a more original form of the root and, moreover, was the form preferred, or even regularly used, in the Old Sumerian period. It is the purpose of the following discussion to prove the correctness of this view.

1. The Attestation of sì(m) = *nadânu*

In order to make ourselves acquainted first with the general aspect of the problem, the fact must once more be stressed that the pronunciations of the root sì(m), "to give," as sì(m) and su_{11}(m) are both attested by Babylonian vocabularies, the former if for the present we confine ourselves to the evidence quoted by Thureau-Dangin, by the equation in S^b, No. 2 (= CT XI 24 f.), col. 3, l. 7,

| si-i | sì | *na-da-nu*

[4] That this is Thureau-Dangin's assumption follows from the fact that he quotes the root exclusively as sum and sûn, not as su_{11}, su_{11}(m) or su_{11}(m). Note, on the other hand, that he gives the root of the Sumerian equivalent for *nadû* as sì and sì(g), i. e., the root sig with dropped final *g*.

and the latter by Chicago Syllabary, l. 123

⌜su-um	sì	" (= *su-nu*)	*na-da-nu*

In his review Thureau-Dangin seems to set great value on the presumed fact that the pronunciation sì(m) of the root sì(m), "to give," is founded only on the quoted passage in S^b, No. 2. However, it can readily be shown that it is found also in S^c: DT 40 (= CT 11, 29 ff.). The partly broken passage, col. 1, l. 80—col. 2, l. 3, of this syllabary, which treats of the sign sì, may be restored as follows:

80 [⌜s]i-i	sì	*su-un-nu*	*ḫa-a-d[i]*
			šu-ut-lu-m[u]
			ta-ma-ḫu
			ba-[.....]
			su-[.....]
			na-[da-nu]
			na-du-[u šá .....]
			da- [.....]
			pa-q[a-du]
			la-mu-u ša [.....]
1 [⌜si]-i	[sì]	*su-un-nu*	*ḫa-sa-..[..]*
			za-na-nu šá [.....]
			sa-pa-[nu]

It will be noted that the first two Akkadian equivalents of sì = sì (written si-i) in this passage, namely *ḫâdu*, "to give," "to bestow," and *šutlumu*, "to give (something to someone)," "to bestow (something on someone)," are direct synonyms of *nadânu*, "to give"; and unless it can be proved absolutely by other evidence that the Sumerian equivalents of the two verbs were different from sì(m) = *nadânu*, we have, of course, to assume that our si-i in the meaning of *ḫâdu* and *šutlumu* is intended to represent the root sì(m), "to give."

The next equivalent *tamâḫu* ordinarily means "to grasp," a meaning, however, which cannot be brought into accord with any Sumerian verb root beginning with sì. But if we assume that this line is taken from a syllabary whose scribe was accustomed to quote the verbs in the I 1 formation even in cases where an exact correspondence would be achieved only by a III 1 form, it would be quite regular. For *šutmuḫu*, "to cause (someone) to take (something)," is again a direct synonym of *nadânu*, "to give." Note especially that it was evidently this parallelism of meaning between *šutmuḫu*

and *nadânu* that has even led to a construction of the former verb which parallels that of *nadânu,* namely, x *ana qati y šutmuḫu,* "to give (or place) *x* in *y*'s hand." Indeed, a direct equation of Sumerian sì(m) and Akkadian *tamâḫu* would result if, with Delitzsch, *Handwörterbuch,* p. 709, a meaning "to give" could be actually assumed for *tamâḫu* itself. But the sense of the passage Aššur-naṣir-apli, 1 R 45 ff., col. 1_{18}, evidently is that Aššur, himself taking or grasping his weapon, stood as helper at the side of the king. At any rate there can be no doubt that in the third line of our passage too sì (= si-i) represents the shortened root sì(m), "to give."

For the seventh equivalent *na-du-*[.....] no other restoration in its first part is possible than the verb *nadû,* which in the texts not infrequently appears as the Akkadian equivalent of sì. From the arrangement of the preserved signs it is clear that the verb *nadû* itself did not fill the line. To all appearances we have, therefore, to restore it to *nadû šá* ....., which means that sì is not the equivalent of *nadû* in all meanings of that verb,[5] but only as used in a special sense or in certain phrases.

As already mentioned, however, Thureau-Dangin in his review assumes that the root of sì = *nadû* is sì(g) and expressly contrasts it with the root sum, sun = *nadânu.*[6] It is not quite clear on what Thureau-Dangin bases this assumption; for, unless I have overlooked some instance in a more recently published text, the only passages in which sì is equated with *nadû* and at the same time is followed by a syllable showing the last consonant of the root of sì = *nadû* are the following.

CT 16, 24 f., col. 1, 29 f.:

[29]ki-za-pa-ág-sì-mu u-me-ni-dé a-daḫ-zu ḫi-a
[30]*a-šar ri-gim*[7] *na-du-ú ú-ru-šum-ma lu re-ṣu-ka*

CT 16, 1 ff., 193:

nam-šub-eridu^{ki}-ga [s]ì-mu-da-mu-de[8]
ši-pat eri-du$_{10}$ *ina na-di-e-a*

[5] This would be possible only in case the line read *na-du-u šá ka-la-ma*; but this is more than unlikely, in view of the fact that the common Sumerian equivalent of *nadû,* as well as the usual Akkadian "ideogram" for *nadû,* is šub, not sì.

[6] Thureau-Dangin: "Il faut lire ì-sum et distinguer nettement sì, sì(g) = *nadû* de sum, sùn (dialecte: zem) = *nadânu.*"

[7] Var. *rig-mu.* [8] Var. sì-mu-dè-mu-dè.

5 R 24 f., col. 4, 6 f.:

[6]ì-da-šè	*a-na na-a-ru*
[7]ba-an-sì-mu	*i-na-ad-du-šu* *

In all of these instances the root of the Sumerian equivalent for Akkadian *nadû* is not sig, but sim. Obviously, therefore, the si-i in the equation

(⌜ si-i) | (sì) | *na-du-[u šá .....]*

cannot represent the shortened root sì(g), but only the shortened root sì(m).

Since thus sì = si-i in the first, second, third, and seventh lines of our syllabary passage represents the root sì(m), there can be no doubt whatever that the whole section from line 1 to line 7 treats of this root; and consequently the broken lines 4-6 of the Akkadian column must likewise contain Akkadian equivalents of the root sì(m). On the other hand, somewhere after line 7 the syllabary should start to give equations in which sì represents the root sì(g); these would then continue through the rest of the passage, for the root of sì = *sapânu* in the last equation is undoubtedly sì(g), as may be seen from the examples given in Br. 4420.

Since, however, the most common Akkadian equivalent of sìm is *nadânu*, "to give," and the passage, as we saw, enumerates several verbs of the same meaning in its first section, it is quite certain that line 6 in this first section must be restored as *na-[da-nu]*. It gives us thus a second instance of the equation sì = sì = *nadânu*, and moreover leaves no doubt that sì in this case represents the root sì(m).

To sum up, therefore: The pronunciation of sì = *nadânu* as sì is attested by two syllabaries (one of the Syll. b and one of the Syll. c class), while that as sum is found in only one syllabary (Chicago Syll. = Syll. c class). If, however, we add the various equations of ⌜ si-i | sì in Syll. c with at least three clear synonyms of *nadânu* and moreover take into consideration that sì = *nadû* also represents the root sì(m), at least six equations attesting the existence of a root form si(m) would compare with the one in the Chicago Syllabary for the root form sum.

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* Sumerian text has "she will be thrown"; Akkadian, "they (= one) will throw her."

2. Evaluation of the Tradition Concerning the Root Forms sì(m) and sum

The differences between the two readings of the verb root sì = *nadânu* in the three syllabaries are the following:

(1). Syll. b, No. 2, and Syll. c give the root without its final *m*, which is amissible and therefore, as in similar cases, can be dropped. The Chicago Syllabary, on the other hand, gives the full root with its final consonant *m*.

(2). Syll. b, No. 2, and Syll. c give the root vowel as *i*, Chicago Syll. as *u*.

a. The Vowel of the Root

Disregarding for the present the difference in the treatment of the amissible consonant at the end of the root, or rather treating the final consonant alike in both cases, the difference between the statements of the three syllabaries can be formulated in this way: Syll. b, No. 2, and Syll. c give the root as si(m); Chicago Syll., as su(m).

In order to evaluate this observation properly we have to remember that it is one of the most common features in the Akkadian syllabaries to give different pronunciations of the same word root. Especially frequent is the pronunciation of a root with *i* in one case, and with *u* in another. For instance, Chicago Syll. in lines 92 and 93

92 ⌜ ki-i	KI	" (= *ki-ku-u*)	*ir-ṣi-tum*
93 ⌜ ku-u	KI	" (= *ki-ku-u*)	"

gives the word for "earth," "land," in the forms ki and ku, although in both cases the word is written with the same sign KI. Note especially that these two pronunciations are found here side by side in the very same syllabary. Compare, furthermore, the parallel word forms

imin and umun, "seven"
ningi and lumgi = *sêbu*(?)
šurun and šurin = *sa-si-ru*

Finally, we may point to the dialectical interchange of *u* and *i* or *e* in word roots, as, e. g., in

uru(d), ES eri(d), "slave"
dug, ES zeb, "knee"
elim, ES elum = *kabtu*
en (< ewen), ES umun (= uwun), ūn, "lord."

Regarded in the light of these observations, the fact that the root of the verb for "to give" appears in the syllabaries in two different forms, namely si(m) and su(m), is not at all surprising, and this fact alone would in no way justify Thureau-Dangin's assumption that one of the two given pronunciations is due to an error.

Moreover, an explanation for the existence of the pronunciation sum beside the pronunciation sim may be readily found. For the *u* of sum can be due to the influence of the labial consonant *m* on the *i* of sim, this vowel being partially assimilated to the *m* by being changed into the labial vowel *u*. This explanation becomes even more likely, if not absolutely certain, when it is observed that the change from imin to umun, ningi to lumgi, elim to elum, and e(me)n to umun was caused, no doubt, by the very same retrogressive influence of the consonant *m* on the vowels *i* or *e* which precede it. The interchange of *i* and *u* as the vowels of the root sìm = *nadânu* can thus be explained as a natural phonetic development.

Obviously because of the observation just made si(m) would appear to be a more original, su(m) a secondary pronunciation of sì, "to give"; and the fact that in Syll. b, No. 2, and Syll. c it is glossed si-i, but in the Chicago Syllabary, su-um, would then merely mean that the former give us the older, the latter, a younger pronunciation.

b. The Amissible Final Consonant

A similar result will be obtained from observations made concerning the treatment of the final consonant *m* of the root sim or sum in the two groups of syllabaries. The very fact that Syll. b, No. 2, and Syll. c, No. 1, give the pronunciation of sì = *nadânu*, the root of which is sim, only as si, that is, without the amissible final *m* of the root, whereas the Chicago Syllabary gives it with *m*, is itself a proof that the reading given by the former syllabaries is more original than that given by the latter. For in old Sumerian, that is, in the times when Sumerian was a spoken language, the amissible consonant at the end of a word was regularly dropped unless it was protected by a following grammatical element beginning with a vowel (GSG § 39). In post-Sumerian times, however, this rule concerning the dropping of amissible consonants was to a large extent purposely neglected in the schools because instruction in, and the use of, Sumerian became much easier by such simplifica-

tion. This observation applies also to the pronunciation of signs or words, as given in the syllabaries; in other words, those glosses in the syllabaries which do not render the amissible consonant at the end of a root reflect more or less the usage and the pronunciation of the time when Sumerian was a living tongue, while those glosses which pronounce the amissible consonant reflect the usage of the post-Sumerian period. Glosses of the first type are quite frequent in the syllabaries and vocabularies, though far less so than those of the second type.[10] Note, e. g., that the equations Syll. c: DT 40 (= CT 11, 29 ff.), col. 2, 3:

(⌜ si-i) | sì | (*su-un-nu*) | *sa-pa-*[*nu*]

and Chicago Syllabary, l. 121:

⌜ si-i | sì | "(= *su-nu*) | *sa-pa-nu*

gloss not only sì = *nadânu*, but also sì = *sapânu*, "to overcome," as si, although numerous passages, e. g., those quoted in Br. 4420, show quite clearly by the "complements" -ga, -gi, and -ki that the root of the verb sì = *sapânu* is sig or sik, not si! In fact, the value sig postulated for sì in former times on account of the fact just mentioned is as yet not given, and probably never was given, in any vocabulary. Similarly in the equation 5 R 21, No. 4, col. 2, 16:

$^{du}_{du_{10}}$ | *ṭa-a-bu*

the Sumerian word for "good" is given as du, although its root, as is well known, is dug.

Especially interesting, however, is the following case. Instead of the equation in CT 11, 23 (= Syll. b, Assyrian), col. 2, 14 (corresponding to K 110, col. 2_{21}), and Weissbach, *Miscellen*, Plate 10 f., col. 2_{22} (Babylonian duplicate):

⌜ mu-ru | MURU | *qab-lum*, "middle," "midst"

the Babylonian duplicate F. 1, Delitzsch, *Assyr. Lesetücke*, 3rd edition, page 64, l. 9, has the equation:

⌜ mu-ru-ub | MURU | *qa-ab-lu*(!), "middle," "midst"

[10] The preponderance of the second kind of glosses is, of course, due to the fact that the vocabularies at our disposition were written in post-Sumerian times.

That muru represents the old Sumerian, murub the post-Sumerian, pronunciation becomes quite evident when we compare with each other the phonetic spellings of the phrases, Gudea, Cyl. A, col. 26, 17:

mu-ru-dingir-ri-ne-ka, "among the gods"

and Rim-Sin, Canephore A, col. 2, 12:

mu-ru-ub-dingir-gal-gal-e-ne-ta, "among the great gods."

In this case the shorter pronunciation has maintained itself in the syllabaries for a remarkably long time; in fact, we find the pronunciation murub as yet in only one duplicate of Syll. b, although in the texts, as we just saw, it was in use as early as the time of Rim-Sin of Larsa.

No less instructive is the rendering of the word pa(b), "first," "brother," "father," etc., in the various syllabaries. The Nippur syllabary, HGT 102 (time of the first dynasty of Babylon), in col. 4, 4 ff.:

4	pa	PAB	*ra-bu-u*
		PAB	*a-ša-ri-du*
6		PAB	*ra-a-du*
		PAB	*a-bu-um*

gives only the pronunciation pa. The younger Nippur syllabary, HGT 104, on the other hand, gives in col. 3, 24 f.:

pa-ab	PAB	*a-*[....]
	PAB	*a-ša-re-*[*du*]

only the value pab. An intermediary stage is found in the two approximate duplicates of HGT 104, namely, Yale Vocabulary, ll. 222 ff.:

222	pa-a	PAB	" (= *pa-ab-bu*)	KA-KA-si-ga
	["]	PAB	"	*a-ḫu*
224	[pa]-ab	PAB	"	*a-ša-re-du*

and CT 35, 1 ff., col. 3, 55 f.:

55	pa-a	PAB	*a-ḫu*
	pa-ab	PAB	*a-šá-re-du* ≷ *a-bu*

It will be noted that the value pab is given only after the shorter value pa, in itself an indication that pa is the older, pab the younger, value. This is definitely proved, moreover, by the observa-

tion that the word pa(b)-gilga, "grandfather," "uncle," literally "the first relation of the father," which is written pà-gi[bi]l-ga and pà-gi[bi]l-ga at the time of Ammiditana (cf. date formula of his 34th year),[11] is written pa-GIŠ-BIL-ga in Eannatum, Boulder A, col. 8, 4, and pa-gi[bi]l-ga in Samsuiluna, LIH 98 and 99, l. 64. Compare also pà-šeš = *ašarêdu*, "first," and *aḫu rabû*, "first (of the) brother(s)," which passed over into Akkadian as the loan word *pašîšu*, "first."

Furthermore, the late Syll. c, No. 1 (CT 11, 29 ff.), col. 4, 33 ff.:

⌜ta-ag	TAG	*šu-ri-du*	*zu-u'-u-nu*
			la-ba-ṣu
			ma-ḫa-ṣu šá mí-ma
			sa-la-ṣu ≷ *ba-a-rum*
			šá-a-lum ≷ *na-ba-ṣu*
			na-du-u

gives, as pronunciation of TAG in the meanings just enumerated, only tag. AO 7661,[12] which is the immediate continuation of the Chicago Syllabary, in column 2, 15 ff.:

⌜ta-ag	TAG	*ma-ḫa-ṣu* ≷ *na-tu-u*
		ra-ka-su ≷ *za-ka-pu*
		si-niq-tu ≷ *ḫu-ut-tu-tu*
		šá šu-TAG-TAG *šu-ta-nu-du*
		šá šu-TAG-TAG *gu-taš-šu-ru*
		šá ú-TAG *in-gu*
		la-pa-tum sá ka-la-ma
		" šá GIŠ.ŠÁ.PA
⌜ta-a	TAG	*šuk-lu-lu* ≷ šá nam-TAG-ga *ár-nu*
⌜da-a	TAG	*šuk-lu-lu* ≷ šá nam-TAG-ga *ár-nu*

likewise gives in its first section the reading tag, but also lists, though only as a sort of addendum, the older values da and ta without the final *g*.[13] Finally, the sign list CT 41, 47, which, with the lists preceding and following it, formed the nucleus of the

[11] See Poebel, *Babylonian Legal and Business Documents* (BEUP, vol. VI 2), pp. 95 f.

[12] Published by Scheil in *Nouveaux vocabulaires babyloniens*, pp. 5 ff., and by Thureau-Dangin in *Tablettes d'Uruk* (TC VI), No. 37.

[13] By this arrangement the syllabarist indicates that at his time the generally recognized value of TAG was tag. Ta and da seem to be listed rather as curiosities, referring to old but still known writings or pronunciations.

Chicago Syllabary and AO 7661 and is of a much earlier date than either of them, enumerates in ll. 85 f.

⌜ ta TAG
⌜ da TAG

only the shorter old Sumerian pronunciations ta and da.

Note, finally, that the same old sign list in l. 87

⌜ še-ri TAG

gives for TAG only the shorter phonetic value šeri, as contrasted with the values šerim and šerid given by AO 7661, col. 2, 27:

⌜ še-ri-im | TAG | šá giš-TAG *ṣi-ṣi-tum*

and col. 2, 33:

⌜ še-ri-id | TAG | KA-KA-si-ga

The value šeri of the old sign list represents the first part of the root šerim as well as the root šerid, just as the phonetic value sì in Syll. c, No. 1, represents the first part of the roots sim and sig.[14]

There can therefore be no doubt that the pronunciation sì given by Syll. b, No. 2, and Syll. c: D 40 for sì = *nadânu*, as far as the treatment of the final *m* is concerned, reflects an older usage than the rendering in the Chicago Syllabary with *m*, a conclusion that is in complete accord with the fact that the vowel *i* of si, as has been shown above, is the original, while the *u* of sum is secondary.

3. Refutation of the Arguments for the Exclusive Reading sum

a. Doubling of the Final Consonant

Having noted these findings, which completely reverse Thureau-Dangin's evaluation of the statements in the syllabaries, we are now prepared to examine the validity of the reasons adduced by him in support of his opinion.

We begin, then, with Thureau-Dangin's general view of the amissibility of final consonants in Sumerian. As far as I know, Thureau-Dangin has never published a comprehensive statement on

[14] The values šerim and šerit seem to have some connection with the Akkadian equivalents of TAG in the equations ⌜ šu-uš | TAG | ...., *sá-ra-mu*, col. 2, 30, and nam-tag-ga = *šêrtum*, Del., HW, p. 636; but owing to the deficiency of the material nothing certain can be concluded.

this question, but he has explained his stand in a letter to the writer, dated August 8, 1928, whose substance is as follows. He recognizes the amissibility of consonants in Sumerian, but believes it necessary that in every special case it should be expressly attested by Babylonian vocabularies, sign lists, glosses in texts, etc., since it is likely or at least possible that certain Sumerian roots end with a double consonant and that such roots probably do not drop both of the two final consonants. To illustrate this point he refers to the French *si* from Latin *sic*, *ami* from *amicum*, and *épi* from *spicum*; but *bec* from Latin *beccum*, *sac* from *saccum*, and *sec* from *siccum*. In the first group the *c* is dropped because the root of the Latin original ended with *one c* only; in the second group, however, the *c* is preserved and pronounced because the root of the Latin original ended with *double c*. In his letter Thureau-Dangin restricts his remarks to words ending with *k*; he therefore had no occasion to mention the root sum, "to give." But his view would affect this root, because its Emesal form zem without any exception doubles the final *m* before vowels; cf., e. g., mudru(?)-zé-em-mu = *na-din ḫaṭ-ṭi* (4 R 9, obv. 34 f. [Haupt, *Die akkadische Sprache*, XXXIX]) and nu-zé-em-má = *ul a-nam-*[*din*] (ASK II, No. 21, rev. 27 f.). Thureau-Dangin, in fact, might even use this Emesal form zem as a very welcome illustration of the rule suggested by him in his letter, because the *m* of zem in the cases where it stands at the end of the word is never dropped; cf., e. g., ma-an!-ze-em | ma-an-sì | *id-di-*[*na*] (5 R 12, No. 1, last column, l. 25).

As early as twenty-five years ago, when I began my collection of words for a Sumerian dictionary, I was obliged to test the very possibility suggested by Thureau-Dangin in his letter. In fact, at that time it seemed to me an axiom that those roots which in the transliteration of Sumerian signs then commonly accepted seemed to be written with a double consonant, e. g., gal in gal-la and dingir in dingir-ra, should be listed with a double consonant at the end, i. e., e. g., gal as gal(l), dingir as dingir(r). But I soon became convinced that it is not possible to prove with any degree of certainty for any Sumerian root that the double consonant found at its end is actually one of its original, and not merely accidental, features. Nor was it possible to prove that the other roots, including even those whose vowel originated from a contraction of two vowels, could not at times appear with doubled final

consonant, at least when using the commonly accepted system of transliteration based on the phonetic values attributed to the Sumerian signs by scribes of the post-Sumerian period. On the contrary, all observations tend to show that a consonant can be doubled or sharpened whenever it follows a stressed vowel and is followed by another vowel. Note, e. g., that the loan word ekil (from Akk. *eqlu*) is written ek-kil in the Chicago Syllabary, l. 280, and that the old Sumerian prefix forms eme- (or imi-) and ema- appear as immi- or imma-, respectively, in late Sumerian and post-Sumerian texts. This observation holds good even for the very latest post-Sumerian periods, as may be seen, e. g., from the fact that (giš)sA$_6$ in Syll. b, No. 2 (= CT 11, 25 ff.), col. 2, 23, is glossed gi-ši-im-mar, but in PSBA 1902, p. 109, 1 f., which dates from the Seleucid period, is rendered as γσιμαρ, with one μ only. All our observations tend to indicate, moreover, that every vowel of a Sumerian root is basically short, apart, of course, from those roots in which a long vowel has resulted from the contraction of two short vowels.[15] Theoretically, therefore, the final consonant of every Sumerian root could be doubled, although, as we shall see, there could also occur the lengthening of the short vowel.

The gathering of evidence in the matter is rather difficult because in the old Sumerian system of writing neither the doubling of a consonant nor the length of a vowel is expressed. But it must be realized that this peculiarity of the Sumerian system of writing, which also passed over into the older Akkadian system (and can even be traced down to the older alphabetic systems), is natural only if the language by which that system was used did not itself distinguish between short and long vowels nor between single and double consonants. The very fact that the Sumerian system of writing shows that peculiarity may therefore be taken as a clear indication that Sumerian actually had no roots with final double consonant or with a basically long vowel.

The same result can be obtained from an investigation of the

[15] Cf. ēn (< *ewen) = ES umun, un, ū(n), "divine lord," sūn < sumun (suwun), "old," dū < dumu, "child," nēn < nimin, "to go or stroll about," ninâ (vocabulary gloss ni-na-a, CT 11, 35 f.: S 1300, col. 3, 6; ni-ná-a, Chic. Syll. 159) and ninua (gloss ni-nú-a, Chic. Syll. 150); cf. *âluninua* < **ninawa* (preserved in Hebr. נִינְוֵה, Greek νινευη, νινευε, and Arab. نِينَوَى). Assyrian *ninua* < **ninawa* like *tuâru* < **tawâru*, *ituar* < **itawar*.

treatment of the vowels and the final consonants in loan words from Sumerian in Akkadian, especially in the names of cuneiform signs, which are formed according to the same rules as those applying to the former. Most of these sign names double the final consonant of the corresponding Sumerian phonetic value of the sign; compare, e. g., *dibbu* (written *dib-bu*)[15a] for the sign DIB, and *tukullum* (written *tu-kul-lum*)[16] for the sign TUKUL. But a number of sign names lengthen the vowel of the Sumerian word instead; compare, e. g., *dûgu* (written *du-u-gu*)[17] for DUG, and *mâšu* (written *ma-a-šu*)[18] for MAS. Yet from the writing of the Emesal root zeb, "good," which is etymologically identical with the root dug of the main dialect, as zebb in zé-ib-ba = *ṭa-a-bu* (K 101, Obv., l. 11), a-ze-ib-ba | a-du$_{10}$-ga | *mê*^{pl} *ṭa-bu-tu* (5 R 11, Col. 2, 27), and su-nu-zé-ib-ba = *la ṭu-ub ši-ri* (Langdon, BPP, pl. 2: K 4664, ll. 8 f.), it is evident that the *u* of the Sumerian root dug, "good," was not long, or at least not basically long. Clearly, the lengthening of the vowel or the doubling of the final consonant in the name of a sign is in no way an indication that the vowel of the corresponding Sumerian root is long or short, nor does it prove whether the root ends with a simple or a doubled consonant. The very fact, however, that the Akkadian sign name either lengthens the vowel of a Sumerian root or doubles the final consonant is a sure indication that the vowels of all Sumerian roots are basically short, lengthening of the vowel as well as doubling of the following consonant being due simply to the influence of accent.

Above, we have briefly referred to the fact that the old Akkadian system of writing, which is based on the Sumerian and is essentially identical with it as far as its basic principles are concerned, agrees with the Sumerian system also in the principle of leaving length of vowel and doubling or sharpening of consonant unexpressed. Since in Akkadian, as in all other Semitic languages, length of vowels was a very essential means of expressing grammatically important differences, it would have been natural for it to invent from the start means of expressing the auditory differences in writing also. Quite on the contrary, however, even those phonetic values of the Akkadian system which were derived from genuine Akkadian words are used with complete indifference as to length of the vowel. The value

[15a] CT 11, 1 f., col. 3, 34.
[16] Yale Syll., l. 113.
[17] CT 11, 1 ff., col. 2, 11-14.
[18] Yale Syll., l. 243.

bit, e. g., for É, derived from Akkadian *bîtum*, "house," and the value *ram* for ÁG (instead of KI-ÁG), derived from *râmu*, "to love," are used to render not only the syllables *bît* and *râm*, but also the syllables *bĭt* and *răm*. On the other hand the value *amat* for GEME, derived from *amtum*, cstr. *amat*, "maid," "girl slave," and the value *šam* derived from *šammu*, "herb," in spite of the short vowels of their prototypes are employed also for *âmat* (e. g., in writing *tiâmat* as *ti-amat*) and *šâm*. The principle underlying this remarkable usage is simply inherited from the Sumerians, who had adopted it because their language did not possess word roots with basically long vowels.

This Sumerian legacy, by the way, extended even into the Akkadian language. Among all Semitic languages, Akkadian alone shows a marked propensity to double the consonant after a stressed short vowel, at least in the forms of its present tense, as, e. g., in *ikáššad* < *ikášad*. This is simply due to the influence of the Sumerian language, in which as a rule the consonant could be doubled after a stressed short vowel. Even more significant is the fact that among the Semitic languages Akkadian alone, to any rather large extent, resorts to replacing a phonetic complex consisting of long vowel and consonant by a complex consisting of short vowel and double consonant, or vice versa. Compare, for instance, *muterru* < *mutêru* (< *mutairu* < *mutaṷirum* < *mutaṷṷirum*); *turrum* < *tûrum* (< *taurum* < *taṷṷurum*); *ḫubullu* < **ḫabûlu*; *ṣûmu* < *ṣummu* (< *ṣum'u*); *bîru* < **birru* (< *bir'u* < *biriu*). This interchange clearly reflects the fact that the Sumerian word roots with their basically short vowels can lengthen the vowel or double the following consonant. These observations, however, again are a valuable corroboration for our conclusions concerning the character of the vowels and the rôle of the doubling of consonants in Sumerian.

Returning now to the question raised by Thureau-Dangin with regard to the amissibility or non-amissibility of the final consonant of a Sumerian root, in the light of the results just obtained we can now state that there are no Sumerian roots which are characterized by final double consonant; such a feature, therefore, cannot be a criterion for the inamissibility of that consonant. Doubling of the final consonant is frequently found in the usual Emesal texts, which favor the broken writing of closed syllables and therefore can unequivocally indicate the doubling of a consonant, whereas the

so-called "ideographic" writing favored in the main dialect is always ambiguous as to whether the "ideogram" should be read with a shorter value—corresponding to the older usage—or a longer value—corresponding to the later custom. But the usual Emesal texts themselves belong in the period characterized by the tendency to restore the formerly dropped consonants at the ends of words. And even if the presence of the final amissible consonant at the ends of words in Emesal should be due to the fact that this dialect never went as far as the main dialect in dropping final consonants, that is, it preserved the final consonant in certain cases, this would prove nothing for the main dialect, to which both the root forms sì(m) and sum—the subjects of this study—belong.

Nor, in the light of the results obtained in our investigation, can the doubling of the final consonant of the Sumerian root in the comparatively late Akkadian and Sumerian sign names be considered indicative of the inamissibility of that final consonant. This point can be established directly from statements of the syllabaries, etc. The sign *pappu* (written *pa-ap-pu*, Yale Syll., ll. 220 ff.), for instance, is not only given the phonetic value pab with preserved *b* at the end, but also, as we saw, the value pa without that *b*. Indeed, the texts show that throughout the older period the sign is read pa, and that only in later, that is, post-Sumerian, times, is the pronunciation pab given. In the same way the sign *siqqu* (written *si-iq-qu*, CT 11, 1 ff., col. 5, 49 f.) has not only the value sig in Akkadian *šig*), but also se_{11} (HGT, No. 124, 3) and (in the old Akkadian) *ši*. Note, furthermore, the values gu_4 and gud of the sign *guṭṭu* (written *gu-uṭ-ṭu*, Br. 5731), lá and lal for *lallu* (written *la-al-lu*, Yale Syll., 207 f.), zà and zag for *zaggu* (written *za-ag-gu*, Br. 6459), gu_8 and qum for *qummu* (written *qu-um-*[*mu*], CT 11, 13: 46287, col. 3_{30}), urì and uru for *urinnu* (written *u-ri-in-nu*, CT 11, 39: K 7668, 1-5), and teme, temen[19] for the Akkadian loan word *temennu*.

Returning to the verb sì, "to give," we can now say, on the basis of the foregoing observations, that neither the doubling of the final *m* of the root zem in the forms already mentioned above, nor the doubling of the final *n* in the sign name *sunnu* (written *su-un-nu*

[19] The strange value timmenna, which is given by Syll. b (CT 11, 14 ff.), col. 5, 43, is probably a loan word from the Akkadian *temennu*, which is itself a loan word from Sumerian.

in CT ii, 29 ff., col. 7, 80 and col. 2, 1, derived from a value sun which Thureau-Dangin thinks is identical with our word sum), can in any way prove that the final *m* of the root was inamissible in the main dialect during the older period. In Emesal, it is true, the *m* of the root zem, "to give," is treated as an inamissible consonant, as is shown, e. g., by the verbal form ma-an(!)-ze-em, "he has given to me" (5 R 12, No. 1, last column, l. 25). Similarly in the post-Sumerian period the *m* of the root sìm of the main dialect could be treated as inamissible, as is proved by the phonetic value sum given in the Chicago Syllabary. At that time, indeed, there is no doubt that some of the scribes were accustomed to pronounce the verbal form ma-an-sì, "he has given me," exclusively as ma-an-sum. But, as we saw, even very late syllabaries still give the old pronunciation *sì* for sì, "to give," which indicates that even in the latest times some Akkadian scribes or certain schools pronounced the root sì, "to give," sì(m). This fact, as well as the reading sì for sì, "to give," in the old Sumerian period, therefore, is in no way affected by the doubling of the *m* in Emesal and the doubling of the *n* in the sign name *sunnu*.

b. The Vowel *u* after the Root

Since Thureau-Dangin does not indicate the details of his argument that the reading sum for sì = *nadânu* is proved by the phonetic "complement" -mu, we are left in uncertainty as to whether he wants to base his argument exclusively on the consonant *m* of the "complement" or also on its vowel *u*. If we assume the former to be the case, his argument would evidently run as follows. As indicated by the *m* at the beginning of its "complement" -mu, the root of sì = *nadânu* must doubtlessly end with an *m*. Now since sum is the only value ending with an *m* that is given as a reading of the root sì = *nadânu* in the syllabaries, this value sum must be considered to be the only possible reading of the root for "to give" to the exclusion of, for example, a reading sim. As a matter of fact, however, the argument is conclusive only as far as the final consonant *m* of the root is concerned; it does not prove that the vowel of the root was necessarily and exclusively an *u*. For the fact that up to the present only sum is found in a syllabary as a value of sì with final *m* may of course be quite accidental; that is, it is not at all inconceivable that some new syllabary will give us a value sim for sì = *nadânu*. In order to assume, against the

testimony of two syllabaries, that a value with the vowel *u* is the only correct reading, it would be necessary to adduce additional evidence which proves conclusively that the vowel of the root of sìm = *nadânu* can be nothing else but an *u*. It is therefore quite possible that Thureau-Dangin did not intend to stop in his argument at the point just indicated, but that he may actually have had in mind to prove the *u* as the only possible vowel of the root for sì = *nadânu* from the vowel *u* of the "complement" -mu. But whatever Thureau-Dangin's reasoning in this case actually was, it will be necessary for us to investigate the question whether the *u* found so frequently after the root sìm is really an indication that the root must be sum.

The *u* found after the root sìm, as, e. g., in ASKT II, No. 1, (*Ana ittišu*, Tablet I), col. 1, in the well-known verbal forms

in-sì-mu	he will give	l. 19
in-sì-mu-ne	they will give	l. 20
in-sì-mu-uš	they have given	l. 18

as well as in sì-mu-dam, translated *i-na-ad-din* in ASK II, No. 2 (*Ana ittišu*, Tablet II), col. 1, 47, is for the usual *e*, as it is found, e. g., in the following forms of the verb lal, "to weigh," "to pay," in ASK II, No. 1, col. 2:

in-lal-e	he will pay	l. 3
in-lal-e-ne	they will pay	l. 4
in-lal-eš	they have paid	l. 2

as well as in *taḫ-ḫi-dam*, translated *i-a-ṣa-ap* (ASK II, No. 2, col. 1, 45). It must be conceded that, in a number of other cases in which the *e* of the grammatical elements following the root changes to *u*, the vowel of that root is indeed an *u*. Compare, e. g.:

nam-ba-gub-bu-de = *la ta-at-ta-nam-za-az* (CT 16, 27 ff., ll. 82 and 84)
ì-šub-šub-bu-uš-ám = *up-ta-az-zi-zu-ma* (King, LIH, No. 99 and 97 f., col. 3, l. 69 [65])
mu-un-dúb-dúb-bu-ne = *i-nap-pa-ṣu* (CT 17, 31 f. Obv. 10)
ì-dub-dub-bu = *ta-bi-ik* (4 R 24, No. 1 Obv. 30 f.)
túm-túm-mu-dè = [*ur*]-*ra-am* (LIH, No. 98 f. and No. 97, Winckler, AK 74, l. 28 [26, 27, and 23 resp.])
ba-gul-lu-uš-a, "which had been destroyed," date formula of the 17th year of Samsuiluna (BE VI, 2, pp. 74 f.)
tuš-ù-dè, "to cause to dwell," VAT 2684 (unpublished inscription of Samsuiluna) [now published AOf IX, p. 241 ff.]
nam-ba-gur-ru-da = *a-a i-tu-ru-ni* (CT 16, 12 ff., col. 4, 36)

ba-an-gúr-ru-uš = *ik-tan-šu-uš* (Haupt ASK II, No. 10, rev., ll. 29 f.)
ib-da-ab-kúr-ru-a, "who alters" (R-S-Š-b, l. 45)
gu-ru-uš bí-in-bu-ru-uš = *iq-ṣu-ṣu-ma* (CT 16, 12 ff., col. 1, 9 and 11)

On account of the comparatively great number of these cases I was myself inclined in GSG § 18 to find the determining factor of the change of the post-radical *e* to *u* in the *u* of the root. However, there are certain roots, e. g., tar and bad (see below), whose vowel is not an *u* but an *a*, and after which nevertheless the *e* changes to *u*. In these cases, of course, the rule would not hold. Far more important, however, is the following consideration:

As may be seen from GSG § 17 f. and my study on The Sumerian Prefix Forms e- and i- in the Time of the Earlier Princes of Lagaš, assimilation of one vowel to another in Sumerian is usually progressive, that is, the vowel which undergoes the change is assimilated to that of the immediately *following* syllable.[20] If the second *u* in such complexes as gubb-u and tumm-u (instead of gubb-e and tumm-e) should be due to the influence of the *u* of the roots gub, tum, etc., the assimilation would in this case be retrogressive, that is, the vowel would have been assimilated to that of the *preceding* syllable. For this reason the correctness of the conclusion that it is the influence of the *u* of the root which causes the change of a following *e* to *u*, must appear rather doubtful.

Of the eleven verbs enumerated above, four—gub, šub, dúb, and dub—end with a *b*; one—*tum*—with an *m*; one—gul—with an *l*; one—tuš—with a *š*; and four—gur, gúr, kúr, and bur—with an *r*. The *b* and the *m* are, of course, labials and can doubtless change an immediately following *e* or *i* into the labial vowel *u*. But *š* and *l* also are, or can be, of a more or less labial character, as is seen from the change of *ištu* (> **iltu*, **uštu*) to *ultu* in Akkadian. The *r* must then be assumed to be of a similar character. It would, naturally, be an *r* which, like the labial *l* and *š*, was pronounced with the lips somewhat pursed. The changing of the *e* or *i* after the roots enumerated above may therefore with good reason be attributed to the influence of the consonants *b*, *m*, *š*, *l*, and *r* at the end of the verbal root instead of the vowel *u* in the middle of the root. This would also offer a satisfactory ex-

[20] For the comparatively rare retrogressive assimilation of a vowel cf., e. g., the change of da and ra to de and re after e in the infixes -e-de- and -e-re- (see GSG § 18).

planation for the fact already mentioned that we find the *u* after roots ending with *r* but not containing the vowel *u*. Compare nam ḫa-ba-ra-tar-ru-da = *li-ru-ru-šú* (CT 17, 34 ff., 39 f.); mu-nu-na-an-tar-tar-ru-da = *u-par-ri-i'* (4 R, 7 f., col. 3, 40 f.); ḫe-ni-íb-tar-r[u-ne] = *li-gaz-zi-*[*zu-sú*(?)] (CT 17, 34 ff., 65 f.); note, furthermore, the form ba-an-bad-du-uš = *is-su-ú* (CT 16, 42 ff., 68, variant), which indicates that in Sumerian there even existed a *d* which was pronounced in such a way that it could influence the following *e* to become an *u* (or, as in this case for certain reasons is more likely, an *o*).[21]

As far as merely phonetic conditions are concerned, we may also quote, in support of the suggested influence of a labial or another consonant on the following vowel, the readings of the following nouns and verb roots:

šanabi, sunabu,[22] "two-thirds"
lím,[23] lam$_4$,[23] limmu, lammu, "four"
elam, elamu,[24] "Elam"
^{d}nammu, "Nammu"
^{d}asari, ^{d}asaru,[25] "Marduk"
gabari,[26] gabaru,[27] "equal," "rival"
dari,[28] daru[29]
gištar,[30] gištur,[31] gišṭur,[32] gešdaru,[33] gešṭuru,[34] gidri,[35]

[21] Compare also the imperative and infinitive forms bad-du bad-du = *i-si ri-e-ki* (Ebeling, KARI, No. 31, rev., 3 f.) and ka-bad-du | *pi-it pi-i* (CT 12, 38: K 244, col. 1, 6); further *u$_4$-sù-du*, "distant days" (CT 15, 17); sù-du-šè, "into the distance," "unto distant days" (Zimmern, SK, No. 199, col. 1, 9, 13), etc.

[22] Compare *su-ur-su-na-bu*, "Sur-Sunâbu," name of the boatman of Utanapištim in the Old Babylonian version of the Gilgameš Epic, Tablet X, = zu(r)-šanabi, "Zu(r)-Šanâbi" in the Assyrian version. The reading zu(r) of the UR- of proper names (cf. UR-^{d}nanše, UR-^{d}nammu, etc.) will be treated in a forthcoming study.

[23] See Thureau-Dangin, HS, p. 20, n. 18.

[24] Syll. a (CT 11, 1 ff.), col. 6, 13 (variant elam).

[25] Gloss a-sa-ru to ^{d}asa-ri, Br. 924.

[26] Written gaba-ri.

[27] Written gaba-ru, HGT, No. 34, col. 5, 14.

[28] AO 8870 (RA XXI, p. 170), col. 2, 21.

[29] Chicago Syll. 203.

[30] Cf. the Sumerian phonetic value giš-tar, gi-iš-tar in various sign names, Br. 5559.

[31] Cf. the Sumerian sign name giš-tu-ra (= gištur-a), CT 11, 27: 81-7-27. 200 (duplicate of Chicago Syll.), rev. 17.

[32] Cf. the Sumerian sign name giš-ṭu-ra (= gišṭur-a), Chicago Syll. 228.

gidru,[36] ES mudra,[37] muduru[38]
zigara, zikara, zigaru, "heaven," "sky"[39]
eš,[40] eššu,[41] "three"
úš,[42] ušu,[43] "dead"
anše,[44] anši,[45] anšu,[46] "donkey"
i_{10}(d),[47] iti,[48] itu;[49] i_{17}(d),[50] ití,[51] itú,[52] "month"
midda,[53] miṭṭu,[54] "weapon"
bad,[55] baṭṭu,[56] "to open" etc.
(giš-)essad,[57] (giš-)essadu,[58] "snare," "trap"[59]

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[33] Cf. the Akkadian sign name *gi-eš-da-ru-u* (< *gešdaru-u*(*m*)), Br. 5559.

[34] Cf. the Akkadian sign name *gi-eš-ṭu-ru-u* (< *gesṭuru-u*(*m*)), Yale Syll. 253.

[35] CT 35, 1 ff. (duplicate of Yale Syll.), col. 4, 21 (gi-id-ri).

[36] Yale Syll. 268 (gi-id-ru).

[37] Yale Syll. 269 (mu-ud-ra).

[38] Written mu-du-ru, M 749, Delitzsch, *Glossar*, p. 96.

[39] Delitzsch, *Glossar*, p. 225.

[40] CT 35, 1 ff. (duplicate of Yale Syll.), col. 4, 27.

[41] Yale Syll. 275.

[42] HGT, No. 102, col. 5, 5.

[43] Syll. a (CT 1, 1 ff.), col. 5, 21.

[44] CT 12, 31: 31177, ll. 3 f.

[45] Syll. a (CT 11, 1 ff.), col. 4, 19.

[46] Ibid., col. 4, 10, and variant of l. 19.

[47] Cf. the frequent i_{10}-da, "per month," and the Sumerian sign name ita (=it-a) in the sign name šá-i-tak-ku-bad-i-du, Syll. b (CT 11, 14 ff.), col. 2, 20.

[48] Syll. b, Fragm. F 1 (Delitzsch, AL, 3rd ed., p. 64), l. 7.

[49] Syll. b (CT 11, 14 ff.), col. 2, 19.

[50] Cf. n. 9.

[51] Syll. b, Fragm. F 1 (Delitzsch, AL, 3rd ed., p. 64), l. 8.

[52] Syll. b (CT 11, 14 ff.), col. 2, 20.

[53] Yale Syll. 136 (mi-id-da).

[54] CT 35, 1 ff. (dupl. of Yale Syll.), col. 2, 39 (mi-iṭ-ṭu).

[55] HGT, No. 102, col. 5, 15.

[56] Syll. a (CT 11, 1 ff.), col. 5, 32 (ba-aṭ-ṭu).

[57] CT 17, 25 f., obv., 14, written ([giš]-)és-sa-ad (=*naḫbalu*); CT 19, 48 f., col. 2, 16, gloss és-sa-ad for ZAG-ḪA (=*išḫu šá* [*nûni*]).

[58] CT 18, 43 ff., col. 1, 50, written és-sa-du (=*naḫbalu*); 2 R 22, No. 1, col. 1, 29, written giš-és-sa-dù (=*naḫbalu*).

[59] In Urukagina, Cones A and B (and + duplicate cone C), and Gudea, Cyl. B, essad denotes a person: "catcher," "taker," "hunter" = *ḫa-bi-lu*, synonym of *ṣaiîâdu*, Gilg. Epic, Tabl. I. Note that the accusative in Urukagina, Cones B and C, col. 8, 22, is written ZAG-ḪA = essa(d), while the subject case in Cones B and C, col. 3, 22, and Cone A, col. 4, 9, appears as ZAG-ḪA-du = essa-du (for essad-e); here, therefore, the -u represents the grammatical element -e, which has been changed into -u by the preceding d. In the ^{d}lamma-essad-e-gú-edin-na of Gudea, Gl. B, col.

[u(d)],[60] uta,[61] utu,[62] "sun"
[^{d}mûa(t)],[63] ^{d}mûati,[64] ^{d}mûatu,[65] "Nabû"
idigin,[66] idigna,[67] idignu,[68] "Tigris."

Almost all of the words just enumerated have, or seem to presuppose, a root without a final vowel. This vowel seems to be of secondary character (see GSG § 89) and therefore must be short and changeable. Furthermore, the final consonants of these shorter roots are *b*, *m*, *r*, *š*, and *d/t*, that is, the very same labial or related consonants which, as was shown above, were characterized by a propensity for changing a following *e* to *u*. Even the *n* of Sumerian idignu must have been of a similar character, since in the Akkadian (*i*)*diglat* it appears as *l*, and in Greek τιγρης as *r*. It seems therefore quite plausible to assume that the final *u* of the words listed above is likewise due to the influence of the preceding consonant.[69] The forms limmu and lammu should be especially noted because they have an *u* after *m*, although the vowel of the root itself is *i* or *a*, not *u*. This would be a case completely analogous to the combination si(m)mu = sì-mu. Note furthermore that the root limm(u), "four," in a late period occasionally seems also to appear as lumm(u),[70] which fact again would be parallel to the appearance of the root sim as sum in late times.

12, 5, the e obviously does not belong to essad; the phrase should be translated: "the essad of the canals and of the Guedinna."

[60] Usually used only for "day."

[61] Syll. a (CT 11, 1 ff.), col. 2, 61 (u-ta); cf. also Thureau-Dangin, LC, p. 68.

[62] CT 25, 25, l. 4 (ú-tú for ^{d}UT); CT 12, 1 ff., col. 3, 41 (ú-tu for ^{d}XX).

[63] Yale Syll. 262 (mu-û-a, there given for PA = *eṭlu* only).

[64] CT 35, 1 ff. (duplicate of Yale Syll.), col. 4, 16 (mu-û-a-ti).

[65] Yale Syll. 263 (mu-û-a-tú).

[66] Syll. b (CT 11, 14 ff.), col. 6, 37, variant (i-di-gìn).

[67] Syll. b (CT 11, 14 ff.), col. 6, 37 (i-di-ig-na).

[68] CT 12, 28, rev. 31 (i-di-ig-nu).

[69] It is at present, of course, quite impossible to form these observations into a grammatical rule. For that would require a much more detailed investigation and one which would distinguish between the various periods, the various dialects, peculiarities of the various post-Sumerian schools, the various systems of writing, etc. An important question that ought also to be taken into account is how far the change of vowels at the end of Sumerian word roots was influenced by the fact that in the latest periods the endings of Akkadian words were no longer pronounced and therefore were used almost promiscuously.

[70] Cf. gišal-zú-lum-ma = *ti-te-nu* and *ra-'i-i-zu*, AO 8870 (RA XXI, p.

Taking all this evidence into account, therefore, we may conclude that the change of an *e* into *u* after certain roots is not caused by the influence of an *u* contained in the root, but by the labial character of the immediately preceding consonant at the end of the root. Indeed, the *u* which in many of the words quoted above represents the root vowel will probably itself—at least in some instances—be of secondary origin and due to the influence of the following consonant. This latter then would have exerted its influence not only on the vowel immediately following, but also on the one immediately preceding.

If we now apply these observations to the root sim, it follows that here too the change of the postradical *e* to *u*, obviously, is not due to the influence of an *u* contained in the root sìm, but is caused by the final consonant *m* of the root sìm. Similarly the fact that in late times the root sìm shows an *u* instead of an *i* as root vowel must be ascribed to the influence of this *m*, the assimilating power of the *m* thus affecting the immediately preceding as well as the immediately following vowel.

In this connection it is quite interesting to note that Cylinder B of Gudea gives the future infinitive form of sìm in col. 6, 14 and 25 as sì-mu-da, but in col. 12, 9, as sì-ma-da. The *a* of the latter form is the result of the usual tendency toward vowel harmony which is effected by a progressive assimilation; that is, the vowel *e* of *sim-ed-a assimilates itself to the *a* of the following syllable da. In simuda, however, this tendency of the Sumerian language has not become effective, and the *e* was therefore in a position to be influenced by the preceding consonant *m*. Similarly we find an *u* in tùm-mu-da (col. 9, 20), and simple *i* or *e* in gi-ni-da (col. 6, 12), du$_{10}$-gi-da (col. 6, 13), pá-dè-da (col. 6, 20), sikil-e-da (col. 6, 24, and col. 9, 6), kù-gi-da (col. 9, 6), and DÌM-e-da (col. 12, 12). Note especially the forms du$_{10}$-gi-da and kù-gi-da, which preserve the *i* despite the fact that the roots contain the vowel *u*. This fact again shows that the *u* in sì-mu-da and tùm-mu-da is caused not by the *u* of the root but by the final *m* of these roots. The great majority of future infinitive forms in Cylinder B, however, change the *e* to *a* by assimilating it to the *a* of the following syllable da. Note especially gur-gur-ra-da

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140), col. 3, 12 f. It is preceded by gišal-zú-1 = *ra-'i-i-zu*, gišal-zú-2 = *šá 2-ta šin-na-a-šú*, and gišal-zú-3 = *šá 3-ta* "(= *šin-na-a-šú*).

(col. 11, 23 and col. 15, 4), gub-ba-da (col. 15, 1, and col. 15, 11), and gub-gub-ba-da (col. 15, 8), the roots of which have an *u* as root vowel and furthermore end with one of the labial or related consonants. It will be observed that with the exception of DIM-e-da (col. 12, 12) all of the forms with *e* and *u* occur in col. 6, 11-10, 2, that is, in the first part of the section, col. 6, 11–col. 16, 2, which contains the future infinitive forms.[71] We may conclude from this either that the two sections, col. 6, 11–col. 10, 2, and col. 10, 2–col. 16, 2, were written by different scribes, who followed different grammatical rules, or that, if both were actually written by the same scribe, he followed in the later part of the whole section other rules concerning vowel harmony than in the first part.

For all these reasons the argument that on account of the *u* so frequently found immediately after the root sìm, "to give," this root must be sum and cannot be sim, is inconclusive.

c. The Sign Name *sunnu*

Thureau-Dangin assumes as root form of sì, "to give," not only sum, but also sùn. Although in his note in AJSL 49 he gives no detailed reasons for this opinion, it may be assumed that at least one of his reasons is the fact that the Akkadian name of the sign is *sunnu* (written *su-un-nu*, CT XI 29 ff., col. 2, l. 3, and *su-nu*, Chic. Syll. 120), and that this name presupposes a Sumerian phonetic value sùn for the sign sì. This fact alone, however, is by no means sufficient proof for the assumption that this phonetic value of sì represents a second root form of the verb sì = *nadânu*. The proof would be complete only if, e. g., in a text passage where there is no doubt that the verb sì means "to give," this verb is followed by a "complement" beginning with *n*. I myself know of only one instance where the sign sì is followed by a syllable beginning with *n*, namely, the name ma-an-sì-na, found, e. g., in Delaporte and Thureau-Dangin, *Catalogue des cylindres orientaux*

[71] Altogether col. 6, 11–col. 16, 2 contains eighty-one LAL-ed-a forms; col. 6, 11–col. 10, 2 contains thirty-seven, nine of which clearly have *u*, *i*, or *e*, and eight clearly *a*, while the remaining twenty cases either do not indicate the vowel in writing or may represent contractions. But we may suppose that at least the forms GUB-da (col. 6, 15) and IL-da (col. 6, 18) were indeed as gu(b)bu-da and ili-da over against the gub-ba-da of col. 15, 1, 11, and the il-la-da of col. 11, 15. Among the forty-four forms of col. 10, 3–col. 16, 2, one clearly has *e*, while thirty-two have *a*.

I, T 154 and 174: ma-an-sì-na[72] | dub-sar | dumu-lu-ka-ni, "ManSIna, the scribe, the son of Lukani"; ITT III 6522, obv. 4: na-ba-ša$_6$-dumu-ma-an-sì-na-ge, "Nabaša, son of ManSIna"; Reisner, TT, No. 185, obv. 6: [u]r-^{d}ba-ú-dumu-ma-an-sì-na, "Ur-Bau, son of ManSIna," etc. But is there any assurance that the root of the verbal form which seems to be contained in the name is sìn < sìm?

That certain Sumerian verbal roots appear with both *n* and *m* as their final consonants is a fact well attested; compare, e. g., girin and girim, "fruit," and other words enumerated by me in MAG IV (1929) 167. In almost all of these cases, however, an original *n* has been changed to *m*, while the change from *m* to *n* seems to occur only in rare and isolated cases. Nevertheless, it may be argued that phonetic developments in one direction are frequently paralleled by a corresponding retrogressive development in other cases, and furthermore one might argue that the root sun, "to give," which Thureau-Dangin assumes, was perhaps the original form, from which the root sum developed by a change of the final *n* to *m*. Obviously, however, speculations of this kind do not furnish any conclusive proof that the combination sìn in the name ma-an-sì-na actually represents the root sìm, "to give." At any rate, in the many hundreds of times the root of the verb for "to give" occurs in such a manner that its last consonant is recognizable, the root is invariably sìm.

Nor is there any conclusive proof that the sign name *sunnu* must have been derived from the root of the verb for "to give," for it would be quite as well possible to assume that the name *sunnu* was chosen because the sign was used for some word root su(n) or si(n) with an *n* as its regular final consonant, and one that had no connection whatever with the root sim or sum, "to give." As a matter of fact, it would be rather remarkable that a sign should be named after a root form which was not recognized as a good pronunciation of sì, "to give." The only plausible explanation, in case the name was actually derived from that word, would be that the name *sunnu* dates from prehistoric times, and that in that early

[72] In the photographic reproduction of the seal impression on Pl. 11, No. 6 (= T 154) only the first part of the name is visible; judging from Thureau-Dangin's transliteration, however, other impressions of the seal on the tablet furnish all of the missing parts of the name. No photograph of the seal impressions on T 174 is given.

time the root of the verb for "to give" was sun, which only later became sum; but with such an assumption we would again venture into the field of unprovable conjectures.

No matter, however, what the real origin of the sign name *sunnu* was, the fact that the vowel of the name is an *u* would not conclusively prove that the word from which it was derived could have been only sun, not also *sin or *sen. This may be illustrated by the name and the phonetic readings of the sign TAG. This sign was known to the Babylonian scribes by the name *šurîdu* (written *šu-ri-du*, Syll. c: DT 40 [= CT 11, 31 ff.], col. 4, 33; Syll. c: 38729 = CT 12, 24, col. 1, 54), which would point to a phonetic value *šurid*. As a matter of fact, however, the syllabary AO 7667 (Thureau-Dangin, TC VI, No. 37), col. 2, as already mentioned, gives as phonetic values similar to that name, the values šerid (written še-ri-id) in l. 33 and šerim (written še-ri-im) in l. 27, and the much earlier list CT 41, 47, l. 87, finally, gives only the value šeri (written še-ri). Thus in spite of the *u* in the first syllable of the sign name the syllabaries give us only pronunciations with *e* in that first syllable.

This sign name *šurîdu* is instructive in other respects also. For although *šurîdu* was the common name of the sign TAG and it might therefore be expected that the value or word šerid was very well known, nevertheless the lists give for šerid only the scribal note KA-KA-si-ga, which, whatever its exact meaning may be, at any rate indicates that the scribes did not know any definite meanings of this word. Note, moreover, that Syll. c: DT 40 gives as reading of TAG only *tak*, although the sign name is given as *šuridu*. It would not be at all surprising, therefore, to find that the phonetic value sun or sin, on which the sign name *sunnu* is based, had no connection whatever with the root sîm, "to give," but was derived from a word sun or sin of some unknown meaning. The sign name *sunnu*, therefore, is not proof for a reading sum. Should some connection between sun and sum actually exist, however, the case would of course stand just as before; sun would then be only a variant pronunciation of sum and, like the latter, itself a late pronunciation of the root si(n) or si(m). A comparison with the sign name *šuridu* makes this point quite clear, because a phonetic value sen, se(n) or sin, si(n) for a sign called *sunnu* is, of course, quite as possible as a value šeri, šeri(d), šeri(m) for a sign called *šuridu*.

4. Summary of Results concerning the Phonetic Values sì(m) and su$_{11}$(m)

Summing up the results with regard to the reading of the root sì = *nadânu*, we can now assert positively that there is no evidence to prove Thureau-Dangin's assumption that sum and sun are the only correct renderings of the root. There still remains the undoubted fact that two Babylonian syllabaries give us sì(m) as the pronunciation of sì(m), "to give," and that only one syllabary thus far attests to the existence of the pronunciation sum. Furthermore, all observations point to the fact that the reading sum, which is given by the Chicago Syllabary, is young, while no such evidence can be adduced for the reading sì(m), the natural conclusion therefore being that sì(m) is at least comparatively a more original pronunciation than sum. The problem presented by the two different pronunciations should therefore not be put, as is done by Thureau-Dangin, in such a way that the answer must decide which of the two is correct and which is not. Both are correct, at least when seen in the light of the historical development through which every pronunciation transmitted to us by the various post-Sumerian syllabaries necessarily must have gone during the long periods covered by the lexicographical tradition of the Babylonians. There are, of course, cases where scribes actually made mistakes, but before assuming such a mistake, it would be necessary to prove that no other explanation is possible. As our investigation shows, this is not the case with the reading sì(m). On the contrary, if it actually were necessary to declare one of the two pronunciations as possibly not genuinely Sumerian, i. e., that it represents a post-Sumerian development, the evidence adduced in this investigation would logically point to the pronunciation sum. As a matter of fact, however, such an assumption would again be far too hasty, since no one can be absolutely certain that phonetic developments observed in the post-Sumerian period did not have parallels already in the various dialects of Sumerian, even in that form of Sumerian which we call the main dialect. For the more we learn from occasional phonetic spellings in Old Sumerian inscriptions as well as from other observations, the more we become aware of the fact that at the time when it was a living language Sumerian, and especially the pronunciation of its roots, offered a far more complicated aspect than it would seem from the more or

less standardized forms transmitted to us through the post-Sumerian scribal schools. With regard to our problem, this would of course involve the very extensive task of establishing the exact forms of pronunciation of the root sìm, "to give," for every locality where Sumerian was spoken, as well as for every period—a task which, however, would require much additional and more dependable material than is available at the present time.

5. A General Survey of the Phonetic Values of the Sign sì

The immediate purpose of this study having been accomplished, it will nevertheless be quite appropriate to round out our investigations by a more comprehensive study of the historical development of the phonetic values of the *sign* sì and the documentation of this development in the syllabaries. Such a study will give us the necessary background for the development of the *root* sìm.

The Chicago Syllabary passage from which the equation sì, "to give" = sum is taken gives the following four equations in the section treating of the sign sì:

⌜ se-e	sì	*su-nu*	*ma-šá-lum*
⌜ si-i	sì	"	*sa-pa-nu*
⌜ za-ar	sì	"	*za-ar-ru*
⌜ su-um	sì	"	*na-da-nu šu-ma*

The first two equations [73]

⌜ se-e	sì	*ma-šá-lum*
⌜ si-i	sì	*sa-pa-nu*

give two readings of the sign sì which resemble each other very closely and are merely differentiated by the closely related vowels *e* and *i*. The similarity between the two equations extends even farther, since the roots of both sì = *mašâlu* and sì = *sapânu*, for which the syllabary gives us the pronunciations se and si, in reality—as we can see from the texts—ended with a *g*, that is, were *seg* and *sig* respectively.[74] On the basis of the equations as we find

[73] We leave out, from now on, the name of the sign.

[74] Cf., on the one hand, nu-mu-un-da-ab-sì-sì-ga = *la maš-lu*, 4 R 9, rev., l. 13 f.; mu-un-da-ab-sì-gi = *u-maš-šá-la*[*-an-ni*], Reisner, SBH, No. 56, obv. 2; *u-maš-šá-lu*, ibid., rev. 55 f., 83 f. (M 2956); nig-sì-ga = *tamšilu* (*tam-šil*), 81, 7-27, 130 (= ZA IV, p. 434), l. 6 f. (M 9289); and, on the other hand, gû-dù-a-sì-sì-ki | *za-bi-in mât za-i-ri*, King, LIH, No. 60, col. 4, 9; bí-ib-sì-sì-gi = *a-sap-pan*, S 954 (Delitzsch, AL, 3rd ed.,

them in the syllabary, it would seem as though the author wanted to distinguish the two roots as seg with *e* = *mašâlum* and sig with *i* = *sapânu*. And it must be conceded that it is quite possible that the two really differed from each other by the *e*- and the *i*-vowel of their respective roots; for in the syllabaries the distinction between *e* and *i* is, on the whole, well carried through. Nevertheless we cannot be altogether sure that this distinction was really intended by the scribes; for we might have here one of those instances where the original syllabary attributed two variant readings indiscriminately to a sign in each of its meanings, but the later syllabaries, in order to avoid tedious repetitions, listed each of the two readings in only one equation. In other words, the two equations quoted from the Chicago Syllabary may have been abbreviated from the following four original equations:

⌜ se-e	sì	*ma-šá-lum*
⌜ si-i	sì	"
⌜ se-e	sì	*sa-pa-nu*
⌜ si-i	sì	"

Be that as it may, it is quite certain that there exists some very close and natural connection between the phonetic values sè and sì of the sign sì; in other words, sè and sì must be or have been simply deviating pronunciations of one and the same phonetic value or Sumerian word. There are various possibilities of explaining how this differentiation of pronunciation might have come about. It may have been quite original; that is, it may date from the very time when the system of cuneiform writing was adopted by the Sumerians. For their language was spoken in various dialects, and a sign used as ideogram for a certain word would, of course, have been pronounced in the main dialect differently than, for instance, in Emesal. Statements to this effect are still found in the syllabaries, which now and then after having given (or before giving) the usual pronunciation of a sign, that of the main dialect, give a second pronunciation with the remark that it is that of the Emesal. Compare, e. g., Yale Syll. 268 f.

⌜ gi-id-ru	PA	" (= *gi-eš-ṭu-ru-u*)	*ḫaṭ-ṭu*
⌜ mu-ud-ru	PA	"	" eme-sal

p. 134 ff., rev. 5 (Br. 4420); sì-sì-ga-e-dè = *ana . . . sa-pa-ni*, 4 R (2d ed.), No. 3, col. 1, 35 f. (Br. 4420); si-ga = *na-as-pan-ti*, Haupt, ASK II, No. 16, rev. 1 f. (M 2160).

and 165 f.

⌜i-ze	UDU	" (= *u-du-u*)	" (= *im-me-rum*) eme-sal
⌜u-du	UDU	"	" (= *im-me-rum*)

In our case, too, therefore, among other possibilities, sè might very well have been the Emesal pronunciation of a certain word, while sì might have represented that of the main dialect.

The other possibility is that the differentiation of the phonetic values under discussion was due to a gradual change of the pronunciation of a certain word written with the sign sì. That is, the sign may originally have had perhaps only the value sè because it represented a certain word se; but, after the pronunciation of this word in the course of time changed to si, the sign sè too might have had attributed to it the pronunciation sì, though it preserved at the same time its old value sè.

It is, of course, quite impossible with the restricted material at our disposal to decide which of the two possibilities really was responsible for the differentiation of the phonetic values sè and sì; it might even be due to both. For our problem, however, this uncertainty is not of decisive importance, since we are mainly interested in getting a clear idea of what sort of connection may possibly have existed between the values sè and sì only in order to illustrate their undeniable original identity.

Applying these observations to the root sìm, "to give," this root too may be assumed to have been originally sem and only later to have become sim. This assumption is indeed very plausible, since a form with the vowel *e* is attested by the Emesal form zem, and since there must obviously have been a period with only one single root form of the verb for "to give" from which both the Emesal form and that of the main dialect originated.

If this was really so, it is evident that instead of the first two equations of the Chicago Syllabary a more extensive and more meticulous syllabary would have given the following six equations:

⌜se-e	sì	*ma-šá-lum*
⌜si-i	sì	*ma-šá-lum*
⌜se-e	sì	*sa-pa-nu*
⌜si-i	sì	*sa-pa-nu*
⌜se-e	sì	*na-da-nu*
⌜si-i	sì	*na-da-nu*

or, in another arrangement:

se-e	sì	*ma-šá-lum*
	sì	*sa-pa-nu*
	sì	*na-da-nu*
si-i	sì	*ma-šá-lum*
	sì	*sa-pa-nu*
	sì	*na-da-nu*

That the Chicago Syllabary does not give the equations se-e and si-i = sì = *nadânu* is, of course, due to the tendency of the syllabaries for utmost economy; it gives for sì = *nadânu* only the reading sum, which at the time when the Syllabary was written and in the school to which the scribe belonged was either the usual or the preferred reading.

This equation sum = sì = *nadânu* is given by the Chicago Syllabary at the end of the section dealing with the sign sì, a fact which under ordinary circumstances indicates that the equation is a late addition. We have seen already that sum can have been adopted as the usual reading of sì = *nadânu* only in post-Sumerian times; but there is nothing to disprove the assumption that a tendency to change the pronunciation of the root from sim to sum already existed in the Sumerian period, although perhaps not recognized by the scribes as the regular or the proper pronunciation. In that early period, however, the sign sì would, of course, have had not the phonetic value sum but only the value su$_{11}$, since *m* at that time was generally treated as an amissible consonant, at least after *i* and *u* (GSG § 40). In the case of sì = *šûmu*, "garlic," etc., one might even be tempted to regard su(m) as a very old reading if there really exists, as is very likely, a connection between the Sumerian word and Akkadian *šûmu*, Hebrew *šûm*, Arabic *ṯûmun*, and Aramaic *tûmâ*. Comparison, however, of Hebrew *sûs*, Aramaic *sûs^{e}jâ*, with Akkadian *sîsû*, "horses," and of Hebrew *bûṣ*, Aramaic *bûṣâ*, Minean *ḅûṣ*, Greek *βυσσος*, "byssos," with Sanskrit *picu*, "cotton," clearly shows that the original Sumerian form of the word for "garlic" could very well have been si(m) instead of su(m). A secondary value su$_{11}$ would also follow from the word su(n) < si(n) or se(n), from which the sign name *sunnu* must have been derived. It is still uncertain, however, at present whether on the strength of the equations sù | *sa-pa-nu* (CT 12, 30: 38179, 8) and sù-sù = *sâpin(um)* (Br. 7605) an occasional read-

ing sù(g) could be assumed even for sì (usual reading sì(g)) = *sapânu*, because at present we have no means of establishing the fact that the root of sù = *sapânu* really ends with *g*. A scribe who wished to include in his syllabary the *old* word forms su(m) and su(n) with their Akkadian equivalents would then have added a third section (cf. p. 73 for the first two sections), which might have read:

⌜su-u	sì	*na-da-nu*
	sì	*šu-mu*
	sì	**šunnu* (or some other word)

If this scribe had then wanted to indicate also the post-Sumerian pronunciations of signs with restored amissible consonants, he might have added, either separately after each single section or in collected form at the end of the whole section dealing with sì, the following equations:

⌜se-em	sì	*na-da-nu, šu-u-mu*
⌜si-im	sì	*na-da-nu, šu-u-mu*
⌜su-um	sì	*na-da-nu, šu-u-mu*
⌜se-en	sì	.....
⌜si-in	sì	.....
⌜su-un	sì	.....

It will be noted that Yale Syllabary 69 gives the Sumerian name of the sign sì, if Clay's copy is correct, as zu-ú-na[75] (i. e., zûna < zun-a). This would indicate a Sumerian value zun, or in older times, with omission of the amissible *n*, zu, instead of the values sun and su which we derived from the Akkadian sign name *sunnu* and the Sumerian sign name su-na (CT 35, 1 ff., col. 1, 61). Moreover, an unpublished school text from Nippur, containing part of the Nippur Syllabary, gives the following readings:

⌜zi-i sì
⌜zu-um sì[76]

Finally, it may be recalled that the Emesal word zem, "to give," the etymologicol equivalent of si(m), also shows a *z* instead of the

[75] In the sign name "(= šá-la-gaba-ku)-zu-ú-na-"(= i-du), = LAGAB with inserted sì.

[76] Read therefore also in the duplicate Langdon, UPUM XII 1, No. 55, col. 8 (= Langdon's col. 6), at the beginning of the preserved portion:

⌜zi-i sì!
⌜zu!-um! sì!

s of the main dialect root forms sim and sum. Thus practically all of the phonetic values of the sign sì which we have yet discussed can be duplicated by values beginning with *z* instead of *s*.

Let us now turn to the equation

⌜ za-ar | sì | " (= *su-nu*) | *za-ar-ru*

in Chicago Syll. 122, because the main dialect reading zar attributed here to the sign sì likewise begins with a *z*.

In order to get a firm basis for a correct evaluation of this phonetic value and its connection with the other values of the sign sì it will be necessary for us to investigate the various groups of equations in

α) Yale Syll. 69-71:

[⌜ za-ar]		" -zu-ú-na- "[77]	*sa-ar-ri šá še-im*
[⌜ .....]		" " " [77]	*za-ra-ru*
[⌜ še-rim][78]	[79]	" " " [77]	*ṣi-ṣi-tum*
⌜ še-rim[78]-sur		"-še-a-" " [80]	*tu-šá-ru šá nap-pil-lum*

and the corresponding passages of its various duplicates, namely,

β) CT 35, 1 ff., col. 1, 58-61:

⌜ za-ar		*sa-a-ru*
⌜ "		" (= *sa-a-ru*) *šá še'i*
⌜ "		*za-ra-ru*
⌜ za-ri-im ! [81]	[83]	*ṣi-ṣi-tum*
⌜ še-dur [82]	[84]	*tu-šá-ru šá nap-pi-i* [85]

[77] I. e., šá-la-gaba-ku-zu-ú-na-i-du.

[78] See note 88.

[79] See note 89.

[80] I. e., šá-la-gaba-ku-še-a-zu-ú-na-i-du.

[81] Copy or text: za-ri-iḫ. Does the original have šá-ri-im ?

[82] See note 89.

[83] Note that this reading of the sign deviates considerably from the usual še-rim-sur. Cf. also note 93.

[84] Instead of the signs še and sì the scribe inserted the Sumerian sign names(!) še(!)-a and su-na. King's copy (and the original?) has KÁRA (= TE + A) instead of še-a in both instances.

[85] For this variant compare CT 14, 9; K 4373, Col. 2, l. 3.

γ) CT 12, 25 f., col. 3, 41-45:

⌜ za-ar	[86]	*za-ar-rum* [ ]
		za-ar-rum [*ša še'im*]
⌜ su-ur [87]	[86]	*za-ra-rum* [. . . . .]
⌜ še-rim [88]	[86, 89]	*ṣi-ṣi-*[*tum*]
⌜ še-rim-sur		*tu-šá-ru* [*ša* . . . . .]

[86] Note that this text inserts the signs ŠE and SÌ not only in the fourth, but in all the signs. Brünnow 10234-36 and 10238 f., Meissner 7754-57, and Thureau-Dangin, LHS, p. 30, note 10; p. 31, note 18; p. 39, note 10, all take the sign to be LAGAB with inserted SÌ instead of LAGAB with inserted ŠE and SÌ; that is, they consider the two inserted signs to be a profuse form of SÌ. To be sure, the fact that in CT 12, 25 according to Thompson's copy the second inserted sign is added to the first without leaving a space between them, seems to indicate that the scribe actually took the two signs as one. Nevertheless, there can be no doubt whatever that the inserted character represents a combination of ŠE and SÌ, as is clearly shown by the fact that it appears also as the writing of še-rim-sur in the last line of the quoted passage and therefore corresponds to the signs of Yale Syll. 71 and VAT 10172, col. 3, l. 33, which certainly consists of LAGAB with inserted ŠE and SÌ (note especially the description of the inserted signs as še-a-su-na, i. e., "ŠE and SUN"). Note, moreover, that VAT 10172 gives the same sign instead of the simple LAGAB + SÌ for *za-a-ru ša* ŠE, a fact which explains the mentioning of *za-ar-ru* (etc.) and *za-a-ru* (etc.) *šá* ŠE in two separate lines; for the inserted simple SÌ (= *zàr*) in the one case corresponds to the simple *za-a-ru*, while the inserted ŠE-ZÀR in the other case corresponds to *za-a-ru ša* ŠE. The scribe of CT 12, 25 f. then apparently took the two different signs simply as variant forms of the same sign and therefore replaced the simple ZAR in the first, third, and fourth lines by LAGAB + ŠE-SÌ. As a consequence he even omitted the sign in the second line, since there was no longer any difference between this sign and that of the first line. Similarly, because they too apparently took the two signs as simple variants, the scribes of Yale Syllabary and CT 35, 1 ff. replaced the LAGAB + ŠE-SÌ of the second line by the simple LAGAB + SÌ, and as a consequence the scribe of Yale Syllabary even dropped the entire first line.

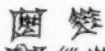
[87] Thus according to Thompson's copy. Brünnow 10236 and 10239 reads zu-ur.

[88] The reading še-rim is proved by Scheil, NVB, p. 5 f., republished by Thureau-Dangin in TC VI, No. 27, col. 4_{28}:

⌜ še-ri-im | TAG | šá GIŠ-TAG *ṣi-ṣi-tum*

[89] Judging from = še-rim-sur, in which sur corresponds to and še-rim to (= RIM with inserted ŠE), it would seem that the sign for šerim alone should be neither RIM with inserted ŠE *and* SÌ (= sur), as it is written above, nor RIM with inserted SÌ (= sur), as it

δ) VAT 10172 (= Assur 3024, not yet published), col. 3, 29-33:

za-ar		*za-a-ru*
"	[89a]	KI-MIN *šá* ŠĚ *sa-ar-rum*
su-ur		*ša-ra-ru* [90]
še-rim [91]	[92]	*ṣi-ṣi-tu*
še-ni-sur [93]		*tu-ša-ru ša* [.....]

It will be noted that CT 35, 1 ff., ascribes to the noun ZAR = *sa-a-ru* (Yale Syll., *sa-ar-ri*; CT 12, 25 f., *za-ar-rum*; VAT 10172, *za-a-ru*) as well as to the verb ZAR = *za-ra-ru* the phonetic value zar, whereas VAT 10172 and CT 12, 25 f. give the reading zar for the substantive only (written LAGAB + inserted sì in one case, and LAGAB + inserted ŠE and sì in the other case), while for the verb *zarâru* (*šarâru*) they give the reading sur$_8$. However since *zarâru* (*šarâru*) and *zarru* (*sarru*) despite the writings *za-a-ru* and *sa-a-ru* are evidently of the same root, there is hardly any doubt that ZAR not only as a verb but also as a substantive had, at least at a certain time, the variant reading sur$_8$, just as the verb was read zar and sur.[94] A value sur, however,

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is written in the other texts. Was the correct sign for šerim RIM with inserted ŠE, which later only by mistake was written RIM + sì and RIM + ŠE + sì? Note, however, that this deduction presupposes the correctness of the gloss še-rim-sur (or its reading, respectively), which to judge from the variants is not altogether certain. See note 93.

[89a] The scribe has inserted into the LAGAB sign only the sign ŠE, not the two signs ŠE and sì indicated by the sign gloss. Note, however, that he placed the ŠĚ into the left half of the LAGAB sign; evidently he intended to write sì into the blank space at the right but forgot to carry out his intention.

[90] Delitzsch, Sum. Glossar, p. 252, reads *da-ra-ru*.

[91] See note 88. [92] See note 89.

[93] Whether the readings še-NI-sur, še-RIM-sur, and še-dur can be phonetically harmonized is still quite uncertain. It is not very likely, since the syllabary glosses use only the most common phonetic values, which undoubtedly are *ni* and *zal* for NI, and *kir*, *kil*, *ḫab*, and *rim* for LAGAB. Possibly the three variants represent synonymous expressions, as, for instance, še-dur, "the dur of ŠE," še-ni-sur, "the sur of ŠE-NI," and še-rim-sur, "the sur of ŠE-RIM." On the other hand, it is quite possible that the differences are due to mistakes of the scribes who copied the texts.

[94] Similarly also gal, "great," at some time must have been pronounced gul, which later appears only as root of the verb gul, "to be (*or* to make) great" (so, e. g., also in the verbal adjective gul-a, written gu-la, gú-la, "great").

must have been attributed also to the simple sì = zàr = *sarru*. For, since the sign LAGAB + sì = zar contains the sign sì, which by itself already expresses the syllable zar, it is evident that the exponent of the readings zar and sur_8 for LAGAB + sì was the sign sì. Note furthermore that in šerimsur also the element sur corresponds to the sì of the compound sign.

With the value sur, zur for sì we gain an immediate contact with the phonetic values zu(m), su(m), and zu(n), su(n), and indirectly also with the values ze, se (= ze(g), ze(m), ze(n), etc.), and zi, si (= si(g), si(m), si(n)). For just as the sign sì with the value zu or su was used to render in writing the first part of the root zum, sum, "to give," just so it could, of course, be used to express the first part of the root zur, sur.

This at once brings up the question whether the value zar which is attested for sì by the syllabaries, or the value zur, which we obtained by a logical deduction, is more original. The value zar is placed by all syllabaries at the head of the section that treats of the sign ZAR. This fact would certainly be significant if it could be established that the syllabarist gives the values in the order of their historical origin. In Chicago Syllabary and in CT 35, 1 ff., zar is even given as the only reading, at least as far as the equivalents for *sâru* and *zarâru* are concerned. Moreover, the loan word(?) *sarru* or *sâru* in Akkadian also presupposes a Sumerian prototype zar, not zur. Sur, on the other hand, appears only as variant of zar = *zararum* in CT 12, 25 f., and the Assur text, and as phonetic reading of sì in the compound šerimsur. The conclusion, therefore, seems to be justified that zur was a secondary pronunciation of later date than zar, just as the pronunciation and root form sum instead of si(m) or se(m) is of a comparatively late date. However, despite the fact that the value zar must be regarded as the more original value, it too in its form zar, with the final *r* pronounced, undoubtedly belongs to the post-Sumerian period. The corresponding pronunciation of the Sumerian period, which dropped the final *r*, must of course have been za only.

But then we are confronted by the question concerning the relation of this value to the values ze or se, which, as shown above, were likewise older than the values zu, su. The most reasonable answer seems to be that za(r) is secondary and goes back to a more original pronunciation ze(r) with an open *e* as its vowel. This *e*

may first have become a sound like the *ä* in German "Fälle," or the *a* in English "fat," and only later on become a more rounded *a*, as, e. g., in German "Wasser." Utilizing the observations made concerning the vowels of certain Sumerian verbal roots in our study on the prefix forms e- and i-, we should assume that the *u* of zur, sur (= *zararu*), which had developed from zar, was, at least originally, also an open vowel, that is, an *o*, the pronunciation of the root then being in reality zor instead of zur.[95]

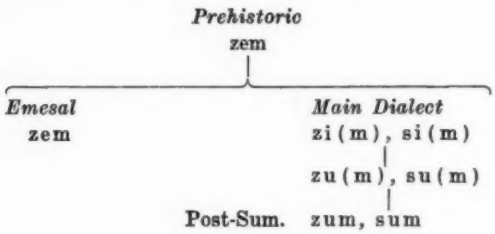
[95] For quite the same reason the root gul, which parallels the root gal, "great," must have been pronounced gol with an open *o* corresponding to the open vowel *a* of gal. As further cases of a pronunciation of *u*, which originated from *a*, as *o*, may be mentioned the nouns gešṭor(u) from geštar(u), and miṭṭo, "weapon," from midda. It will be noted that the Akkadian scribes in the case of geštor(u) (written geš-ṭu-r . . , Chicago Syll. 228; giš-ṭu-ru, Yale Syll. 253), and miṭṭo (written mi-iṭ-ṭu, CT 35, 1 ff., col. 2, 39) render the syllable *ṭo* with the Akkadian sign *ṭu*, the emphatic *ṭ* of which will naturally cause the following *u* to sound more or less like *o*. Also the participle bad-du (< bad-a) must have been pronounced baṭṭo; note the writing of the phonetic value of BAD as baṭ-ṭu in Syll. a (CT 11, 1 ff.), col. 5, 32. For the existence of an open *o* in late Akkadian, compare, e. g., the rendering of *Nabû-kudur-uṣur* with *ναβουχοδονοσορ* (LXX) and *ναβουχοδονοσσορ* (Lucian) in Greek, and נְבוּכַדְנֶצַּר (and sim.) in the Massorah. For transition of *a* into *o* in late times cf. Greek *κυρος* (and Mass. כּוֹרֶשׁ) from Akk. *Kûraš*. For the pronunciation of *a/u* as *o* in Sumerian in early as well as late times, cf. domu, "child," and its secondary form damu (gloss da-mu in A^{e}-da-mu_{DUMU}; see AJSL LI [1934/35], p. 169); the frequent dialectal ṭomu (written ṭu-mu, e. g., 4 R 27, No. 1, 6); the rendering of dumu with *δομ* (with the usual dropping of a final short vowel in late times) in gišgišimmar-domu-domu | *ta-a-lu* = *γισιμαρ δομ* ⟨*δομ*⟩ *θαλ*, PSBA, 1902, p. 109, 1; Hebr. תַּמּוּז, the god and month "Tammûz," from Sum. da(m)mu-zi (< damu-zi(d)) or ta(m)mu-zi; Syr. ܬܡܘܙ, (sometimes also ܬܡܘ = *ta(m)mûz*), "month of Tâmûz," from Sum. tamu-zi. The *t* of Hebrew and Syriac points to a loan directly from Sumerian at a very early time. (For such borrowings directly from Sumerian see Poebel, "The Name of Elam," in AJSL 48, [1931/32], 20 ff.) For old Sumerian *t* instead of late Sumerian *d*, note, e. g., the use of DUMU (= tumu) as equivalent of tu-mu in the old text AO 4331 + 4335 (Thureau-Dangin, NFT, p. 206) see my transliteration and translation in Sumerische Untersuchungen II, pp. 12 ff. (= ZA, N. F., III, p. 38 ff.), col. 3, 6 (Zimmern's assumption in *Tamuz*, p. 703, of a secondary Semito-Babylonian pronunciation *tamûzu* lacks proof); further, the use of *da*, *du*, etc. for *ta* and *tu* in the older Akkadian system of writing, a use that must, of course, go back to the Sumerian system, which the Akkadians merely adapted to the needs of their language. The doubling of the *m* in Hebrew and the length of the first vowel in Syriac,

The results of our investigation of the phonetic values of the sign sì can now be summarized by the following table of old values:

ze, se	rendering	the	root	ze(n), se(n)
				ze(m), se(m)
				ze(g), se(g)
zi, si	"	"	"	zi(n), si(n)
				zi(m), si(m)
				zi(g), si(g)
zu, su	"	"	"	zu(n), su(n)
				zu(m), su(m)
za, sa	"	"	"	za(r), sa(r)
zo, so	"	"	"	zo(r), so(r)

As will be seen, the various phonetic values of sì range through the whole scale of vowels. This is of course not due to independent development of the original phonetic value of the *sign* sì. Phonetic values as a rule tend to remain stationary. The real cause was the change in course of time of the spoken Sumerian *words* that were written with the sign sì.

We may conclude this study by the following schematic representation of the development of the forms of the verb sìm, "to give," as it seems to result from our investigations:



as well as the length of the *u* in Hebrew and Syriac, are easily explained by what has been said of the Sumerian roots on page 52. Note that Jensen as early as ZA, I, p. 17, had correctly explained dumu as domu.

HEBREW "HELMET," A LOANWORD, AND ITS BEARING ON INDO-EUROPEAN PHONOLOGY

EDWARD SAPIR
YALE UNIVERSITY

AMONG THE Biblical Hebrew words which have long been suspected of being loanwords rather than derivable from Semitic roots is the word for "helmet," *kōḇáʿ*, construct state *kôḇaʿ*, plur. *kôḇāʿîm*; a parallel form is *qōḇáʿ*, construct state *qôḇaʿ*. The occurrences are:

1 Sam. 17:5: *wᵉḵôḇaʿ nᵉḥóšeṯ ʿal-rōšó* "and he [Goliath, the Philistine] had a helmet of copper upon his head"

1 Sam. 17:38: *wᵉnāṯán qôḇaʿ nᵉḥóšeṯ ʿal-rōšó* "and he [Saul] put a helmet of copper upon his head" [var. lect. *kōḇaʿ*]

Is. 59:17: *wᵉḵôḇaʿ yᵉšūʿá bᵉrōšó* "and (he put on) a helmet of salvation upon his head"

Jer. 46:4: *wᵉhiṯyaṣṣᵉḇú bᵉḵôḇāʿîm* "and stand forth with (your) helmets"

Ezek. 23:24: *ṣinná umāḡén wᵉqōḇáʿ yāśîmū ʿāláyiḵ sāḇîḇ* "buckler and shield and helmet shall they set up against thee round about"

Ezek. 27:10: *māḡén wᵉḵōḇáʿ tillū-ḇāḵ* "shield and helmet they hung up in thee"

Ezek. 38:5: *kullām māḡén wᵉḵōḇáʿ* "all of them [Persia, Ethiopia, and Libya] with shield and helmet"

2 Chron. 26:14: *wayyāḵɛn lāhém ʿuzziyyāhū lᵉḵol-haṣṣāḇā́ māḡinnîm urmāḥîm wᵉḵôḇāʿîm wᵉširyōnôṯ uqšōṭôṯ* "and Uzziah prepared for them, for all the host, shields and spears and helmets and coats of mail and bows"

The word *kōḇáʿ* lingers on in post-biblical Hebrew with the meanings of "helmet" and "turban," also "thyroid cartilage, Adam's apple" (obviously a metaphorical transfer). It was doubtless in popular use in the Aramaic dialects, as we may judge from the occurrences of *kōḇᵉʿā́* and *qōḇᵉʿā́* in the Jewish Aramaic Targums, again in the two senses of "helmet" and "turban," more particularly "priest's turban," and from the Syriac *qūḇᵉʿā́* (or *qubbᵉʿā́*?[1])

[1] According to Nöldeke. See Ges.-Buhl, sub *qōḇáʿ*.

"cowl, cape, hood" and, again in a transferred sense, "capital of a column." (The Syriac *kūḇā'â* "helmet," on the other hand, is perhaps merely a bookish copy of the Biblical Hebrew *kōḇá'*.) Jewish Aramaic *kōḇ'â*, *qōḇ'â* and Syriac *qūḇ'â* (*qubb'â*) do not agree on the basis of a common Semitic form and meaning (say **kauba'-u* or **qauba'u* "helmet") but on the basis of a secondary borrowing of a West Aramaic *qōḇá'*, with suffixed article *qōḇ'â*, "turban" (m.; cf. plural *qōḇ'ín*, with article *qōḇ'ayyâ*) as *qūḇ'â* (*qubb'â*). The older meaning of "helmet" was no longer strictly applicable in folk Aramaic, one may guess, and the Palestinian Aramaic *-ō-* was replaced in the borrowing Syriac by its nearest phoneme *-ū-* (with variant *-u-* followed by doubled consonant). Had the Syriac form been a true development of the **qauba'u* which lies back of Hebrew *qōḇá'*, we should have expected a Syriac **qaub'â* (cf. Syriac *maut-â* "death": Jewish Aramaic *mōṯ-â*: Hebrew *mōṯ* < Sem. **maut-u*). It is on the basis of a specifically Syriac *qūḇ'â* > *qubb'â* that the Arabic culture loanwords *qub'-u*, f. *qubba'-at-u*, "hood" are most easily understood; Ethiopic *qōbĕ'* too, with its *-ĕ-* (or zero) instead of original Sem. *-a-*, is not directly traceable to **qauba'u* but to some Aramaic prototype. The word seems to have no Accadian cognate. The actual Semitic occurrences are, therefore, limited, in effect, to Hebrew and Western Aramaic, with subsequent borrowings in Eastern Aramaic (Syriac), Arabic, and Ethiopic, and do not suggest a common Semitic word, but a specifically Hebrew word. This lingered on, mostly with the transferred meaning of "turban" rather than "helmet," in the Palestinian Aramaic dialects which superseded Hebrew and had borrowed the word from it. And, in turn, a lone Hebrew word for an object of such distinctive cultural connotations as "helmet" is likely to be of foreign origin.

The most likely guess would be that the word was borrowed from a language possessing a voiceless *k* that was unaspirated and was not strictly identifiable with either the normal Semitic *k* (ordinarily aspirated when non-emphatic: *k'*) or its emphatic correspondent (ordinarily velar and unaspirated when emphatic: *ḳ* = *q*). Such a *k* would have to belong to a non-Semitic language; e. g., the Greek *κ* or, presumably, the Hittite fortis *k* (*k-*: *-kk-*) would be a case in point. Obviously, an original **kauba'-*, with unaspirated *k*, agrees with Semitic *k* (= *k'*) in position but with Semitic *q* in its lack of aspiration. It is well known that at a much later period a

host of Greek words with κ were borrowed by Jewish Aramaic and Syriac with change of κ to *q*. The Hebrew variants *kōḇáʿ* and *qōḇáʿ* are suggestive internal evidence for the non-Semitic origin of the word and it was precisely this alternation that led Barth [2] to speak of the word as of doubtful origin. The formative type to which it would have to be referred as a native Semitic word (*qautal*) is very sparsely represented in Hebrew and the radical from which it might be derived (*kbʿ* or *qbʿ*) either non-existent in Hebrew or clearly irrelevant (*qāḇáʿ* "to rob one of something"). A variant *kbʿ* of the common radical *gbʿ* "hill" is most unlikely.

Philistine, a non-Semitic language contiguous to Hebrew on the west but originally spoken in Asia Minor, is suggested by the first of our biblical citations as a possible source.[3] If this is correct, it would indicate the presence in Philistine of a voiced laryngeal or, more likely, velar spirant: γ (= Arabic ġain) rather than ʿ (Arabic ʿain). The prototype of the Hebrew word would be something like **kaubaγ*-. The cultural evidence points to the helmet, in various forms, as originally more properly at home in Asia Minor than in Palestine.[4] We are, therefore, not unprepared for Hittite **kŭpaḫis* (acc. *kŭpaḫin*, pl. *kupaḫius*) "hat, cap?" [5] The *-ū-* or *-u-* of Hittite *kūpaḫi-* may go back to an IE *au* (cf. Hit. *ḫuḫḫas* "grandfather," Latin *avus* < IE **xauxos*),[6] while the single *-p-* points to a lenis stop,[7] which would be heard by the Semitic ear as *-b-*. The word may have come to Hebrew directly from Philistine or it may have gradually worked its way south from some dialect (with *-au-* for Hit. *-ŭ-*) of the Hittite-Luwian group through Syria, in which case the Western Aramaic forms quoted above represent a culture borrowing from the north rather than from Hebrew.

The most interesting phonologic point raised by the comparison of Hittite *kŭpaḫi-* with pre-Hebrew **kaubaʿ*- (**kaubaγ*-) is the correspondence of Hittite *-ḫ-*, written single, with Hebrew *-ʿ-*. As is well known, Hebrew and Aramaic ʿ represents a pool of Semitic ʿ

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[2] *Die Nominalbildung in den semitischen Sprachen* § 38a2, note 2.

[3] See, e. g., A. S. Macalister, *The Philistines* 80.

[4] See, e. g., Kurt Galling, *Biblisches Reallexikon*, sub "Helm"; M. Lidzbarski, *Ephemeris fur semitische Epigraphik* II. 135 (Shardana warriors); P. Thomsen in Ebert, *Reallexikon der Vorgeschichte* V. 297.

[5] E. H. Sturtevant, *A Hittite Glossary*, 2nd ed., 83.

[6] E. H. Sturtevant, *A Comparative Grammar of the Hittite Language*, 101.

[7] *Ibid.*, 73-83.

(voiced laryngeal spirant) and γ (voiced velar spirant), kept apart in Arabic as ʿ and ġ respectively. The primary value of Hittite *ḫ* was probably that of a voiceless velar spirant (*x*), as in Accadian, and this value would normally be rendered in a Hebrew loanword as *ḥ*, e. g. *ḥittî* "Hittite": Hit. *ḫatti*. I have pointed out elsewhere, however, that in certain cases Hittite *ḫ* must, in all probability, be interpreted as the corresponding voiced (or lenis) velar spirant γ [this rather than ʿ], e. g. *ḫapatis* "vassal" [read *γaʙaɒis*[8]]: Canaanite **ʿabadīma* "slaves" (Hit. γ as nearest native phoneme to represent Semitic ʿ);[9] *Tutḫaliyas*: Hebrew *Tiḏʿál*[10] (Hit. γ borrowed as γ > Can. ʿ).

In the example before us a Hittite *ḫ* is found in intervocalic position. On general principles we should expect such a *-ḫ-* to have the value of a voiced spirant, *-γ-*, in contrast to the voiceless value of *-ḫḫ-* (e. g. *paḫḫur* "fire" = *paxur*), for, as Sturtevant has shown, doubled stops in intervocalic position are voiceless (probably voiceless fortis), while single stops are voiced (more probably voiceless lenis). In other words, *-p-* (written *-pp-*) : *-ʙ-* (written *-p-*) as *-x-* or *-xx-* (written *-ḫḫ-*) : *-γ-* or lenis *-x-* (written *-ḫ-*). Very likely *-zz-*: the less common *-z-* represents a similar contrast between *-c-* (fortis voiceless affricate *-ts-*) and *-ʒ-* (lenis voiceless affricate *-ɒz-*). The pair *-ss-*: *-s-* too may mean something more than a purely orthographic variation. The problem of the meaning of Hittite single and doubled consonants would seem to deserve further study. If we are right in reading Hit. *kŭpaḫi-* as *kŭʙaγi-* < **kauʙaγi-*, we are not far from discovering an actual example of the posited IE γ (Kurylowicz's *ə*[3]) in a recorded Indo-European word of an Indo-European language, for it would be difficult to disconnect pre-Hittite **kauʙaγ-i-* (with derivative *-i-*: *-yo-* suffix?[11] *-ʙ-* softened from IE *-p-* after *w*?[12]) from the large group of IE words represented by Germ. **χauƀu-đ-a-* "head" and Latin *capu-t* (dissimilated from **kaupu-*?). The intricate phonological problems that are further suggested by this (e. g., Latin *cappa* < **kapγ-ā* <**kaupγ-* because of analogy of *caput*?) are not our present concern.

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[8] ʙ, ɒ, ɢ are here used for lenis voiceless stops.

[9] See *Language* 10, 274-79.

[10] *Ibid.*, 276-77.

[11] Cf. Hittite *kesris* "glove": *kessar* "hand."

[12] Cf. JAOS 56. 281.

Sturtevant's interpretation of Hittite *-eḫ-* before vowels (*seḫur* "urine"; *meḫur* "time"; *weḫ-* "to turn") as an orthographic method of representing an IE prevocalic **-e'-*[13] is not consistent with our theory, for it is difficult to believe that Hit. single *-ḫ-* betwen vowels could mean both *-γ-* and *-'-*. In another connection some evidence, of a different type from that here presented, will be given that makes it possible to interpret Hit. *-eḫ-* not as *-e'-* but as *-eγ-*. In other words, it will be suggested that while IE. **-ax-* regularly remains in Hittite as *-aḫḫ-*, IE **-eγ-* (at least before vowels) similarly remains as *-eḫ-*; *-e-* of theoretical IE **-eγ-* does not undergo a change to pre-Hittite **-ǫγ-*[14] (in most IE dialects pre-consonantal **-eγ-* becomes **-ǫγ-* > heavy-base **-ō-*).

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[13] *Language* 12. 186-87.

[14] A number of other cases have been noted of Hittite *-ḫ-*: Canaanite *-'-*. These will be discussed at another time.

THE JAPANESE *SHŌEN* (荘園), OR MANOR: SOME USEFUL TERMINOLOGY[1]

ROBERT K. REISCHAUER
PRINCETON UNIVERSITY

THE TERM *shōen* is composed of two parts. *Shō* means a *bessō* (country home), while *en* means well-kept grounds. These grounds may be parks or cultivated farm lands. In other words, then, a *shōen* was a country estate or manor.

A typical *shōen* consisted of a core of rice fields, surrounded by dry fields, in a section of which were clustered the farmers' huts, with a fringe of waste land forming the outer border and stretching away to the next agricultural settlement. The *shōen* gorged themselves on waste land, turning large tracts into *konden* (new rice fields), and into dry fields, and then often divided like amoeba. Where civilization was highest and the population the thickest, *shōen* generally tended to break down into small units composed primarily of intensively cultivated rice fields. Where the population was sparse and backward, and where agricultural methods were comparatively primitive, however, the *shōen* were quite large, contained a great deal of waste land, and produced rather little per acre of rice and dry fields.

Shōen originated in several ways. A few came from *tadokoro* (private estates) of clan aristocrats that had never been incor-

[1] Most of the material for this paper was obtained from the following three sources: (1) *The Development of the Manorial System* (*Shōen sei no hattatsu*), by Nishioka Toranosuke; published by the Iwanami Shoten, Tōkyō, 1933, in its series of *Iwanami Lectures on Japanese History* (*Iwanami kōza, Nihon rekishi*); (2) *The Organization of the Manors in the Heian Period* (*Heian-chō jidai ni okeru shōen no soshiki*), by Kawakami Tasuke; in Nos. 3 and 4, Volume XI of the *Journal of History* (*Shirin*), July and October numbers, 1926; published by The Historical Society (Shigaku kenkyū kai), The Kyōto Imperial University; (3) *The Manorial Policy of the Heian Court* (*Heian-chō no shōen seisaku*), by Kawakami Tasuke; in Nos. 2, 3, 6, and 7, Volume XXXIV, February, March, June, and July numbers, 1923; in *The Historical Magazine* (*Shigaku zasshi*), published by The Historical Society (Shigak-kai), The Tōkyō Imperial University. This study is confined to the period 645-1165 A. D. No attempt has been made to treat the *shōen* in the Kamakura and Ashikaga periods.

porated in the *handen* (allotted rice fields) system. Many were composed wholly or in part of *jiden* (temple rice fields) or of *shinden* (shrine rice fields), as these were *fuyusoden* (tax free rice fields) that had been granted by the government in perpetuity to the great Buddhist temples and Shintō shrines. Some grew up from *kōden* (merit rice fields), for although these were *yusoden* (taxable rice fields), *daikōden* (great merit rice fields) were granted in perpetuity, *jōkōden* (superior merit rice fields) could be inherited by the receiver's sons and grandsons, and *gekōden* (inferior merit rice fields) passed on to the original receiver's sons. *Daikōden*, therefore, were really private property from the time they were granted, while *jōkōden*, and to a less extent *gekōden*, often became so because they were frequently never returned to the government. Then there were *shiden* (gift rice fields) presented to men for some special work they had done. These were held for life and were sometimes handed on to descendants, thereby easily becoming private lands. *Iden* (rank rice fields) were granted to officials of the *Go-i* (Fifth Rank) and above for as long as they held such ranks; but as these officials were seldom demoted or deprived of their ranks, they held such *iden* for the rest of their lives, and the lands were frequently transferred to sons of high rank on the deaths of the fathers. Consequently, many *shiden* and *iden* soon became private property in all but name, and found their way into the *shōen*. The same thing held true for the *shikibunden* (office rice fields), only it was even easier for these to be incorporated in *shōen*, as they were *fuyusoden*. There were several other kinds of tax free rice fields that were easily changed into *shōen*, such as the *ekiden* (biennial rice fields), termed such because they were so poor that they grew a crop only every other year, the *unemeden* (Palace Women's rice fields) held by the *Uneme* (Palace Women) who served in the *Kōkyū* (Women's Quarters of the Imperial Palace), the *ekiden* (post rice fields) given to those who served on the empire's post roads, the *shaden* (archery rice fields) bestowed on those who had shown proficiency with the bow, and so forth. All these types of rice fields, either because they were held for long periods of time, or because they were tax free, turned rather easily into *shōen* whenever the government relaxed its hold on them. Most *shōen*, however, were composed of ordinary *kubunden* (sustenance rice fields) that had been commended to the owner of the *shōen* by the farmers who

had been allotted these lands under the *handen* system, and of *konden* that had been reclaimed from waste land by wealthy landowners who had the necessary capital to invest in such undertakings. The government lost its hold on the *kubunden* when it stopped taking a census every six years and re-allotting lands at that time, while it ceased to control waste lands when it permitted private individuals to reclaim unlimited amounts and to till these *konden* without paying taxes on them for long periods of time.

The farmers that tilled the fields of the *shōen* were as different in their origins as were the lands they cultivated. Some were descendants of *nuhi* (slaves) that had belonged to the clan ancestors of the lord of the *shōen*, while some were runaway *nuhi* from other estates. Some were farmers who had fled from the imperial lands and abandoned their *kubunden* because of excessive taxation, some had been forced to seek employment in the *shōen* because they had had the misfortune to be born in over populated sections of the country where the available agricultural land was so limited that the government had been unable to grant them their rightful amount of *kubunden*, and some were wanderers by nature who drifted from one *shōen* to another. Many were members of *jikifu* (sustenance households) that had been granted to men of high rank as *ifu* (rank households), *shikifu* (office households), or *kōfu* (merit households).

The ultimate object in establishing a *shōen* was to withdraw its lands from the public domain and have them declared private property, get its farmers struck off the government lists and recognized as people belonging to the manor only, win exemption from the *so* (tax of rice on paddy fields) for all rice lands in the *shōen*, and exemption for all males living in the *shōen* from the payment of *chō* (tribute in kind), *yō* (commuted tax in place of forced labor), from rendering *edachi* (regular annual forced labor), and *yōeki* (additional forced labor), from having to accept *suiko* (loans) from *Kokushi* (Provincial Officials), and from being conscripted for military service, and finally to win the right to prevent all government officials from entering the *shōen* for any reason whatsoever. When recognition of all or at least most of these rights had been won from the *Kokushi*, the *shōen* was called a *kokumenshō* (provincially exempted manor), and when these rights had finally been recognized by the *Dajōkan* (Great Council

of State), the *shōen* was known as a *chokumenshō* (imperially exempted manor). A *chokumenshō* that had won all the above mentioned rights or privileges was, therefore, a completely independent political and economic unit. It was a state within the state.

The waste land of a *shōen* was used in common by all the *shōmin* (people of the manor), but the rice and dry fields were held by individuals. The common farmers were known as *yoriudo* (villeins). There were two types of *yoriudo*, those who tilled *kubunden* they had entrusted to the *Ryōshu* (Lord of Manors) and hence were owners of these fields, and those who had come as landless laborers to the *shōen* and had been given *konden* to till, and therefore were renters. *Shōen* frequently contained estates owned by *Jinushi* (Land Masters), who were sometimes called *Tato* (Rice Field Masters). These *Jinushi* or *Tato* were landowners who were not powerful enough to become *Ryōshu* themselves, but entrusted their lands to *Ryōshu* and then served as officials of the *shōen*. In almost all cases they had *yoriudo* tilling most of the soil for them, although some of the fields they held directly, these being known as *tsukuda* (directly held rice fields). On these *tsukuda* the *Jinushi* generally paid nothing in the way of taxes to the *Ryōshu*, the income from such lands being considered as his salary as an official of the *shōen*. *Shōen* that were composed solely of *konden* had no such *Jinushi*, and officials in that type of manor held *kyūden* (tax free salary rice fields) instead of *tsukuda*.

The central figure in the whole *shōen* system was the *Ryōshu*, or Lord of Manors. His rights in the land were threefold, namely, those of disposal, administration, and taxation. He was entitled to several forms of revenue from his *shōen*. These consisted of the whole harvest from his *tsukuda*, *jishi* (land rent) from *konden* tilled by *yoriudo* as renters, *nengu* (annual tribute) from fields cultivated by *yoriudo* as petty landowners, *nengu* from all the fields of *Jinushi* except their *tsukuda*, and forced labor from all the *shōmin*. In addition, he could levy special payments and demand special services on occasion. The more warlike of the farmers and *Shōshi* (Manorial Officials), moreover, were required to render police and military service.

The administrative officials required to govern these *shōen* were generally known as *Jōshi* (Upper), *Chūshi* (Middle), and *Geshi* (Lower Family Officials). Starting at the bottom of the scale,

the *Geshi*, who were also called the *Shōshi*, or Manorial Officials, consisted of the *Jinushi* or *Tato*, those officials who held *kyūden*, and the more capable and warlike *yoriudo* among the ordinary *shōmin*. These officials represented the interests of the people of the manor and were entrusted with the ordinary administration of the *shōen*. In time, as the *Ryōshu* came to own many *shōen* scattered all over the country, it was found necessary to send *Chūshi* to the *shōen* to look out for his interests and to see to it that the *Geshi* or *Shōshi* were sending him the right amount of taxes and carrying on the work of the *shōen* properly. These *Chūshi* generally set up *Azukaridokoro* (Manorial Managing Offices), within the more important *shōen* and took over the general supervision of the manors. They usually received taxes from certain lands within the *shōen* as their salaries.

At the top of the administrative scale were the *Jōshi*. They met at the main residence or headquarters of the *Ryōshu*, and if he was a *Kugyō* (High Court Noble) and the head of a great family, these *Jōshi* were called *Kashi* or *Keishi* (Family Officials) and formed a large part of the personnel of the *Mandokoro* (Supreme Administrative Council), the *Hyōjōshū* (Supreme Advisory Committee), the *Monchūjo* (Supreme Court), and the *Samuraidokoro* (Supreme Military Council), which had charge of conducting the *Ryōshu's* multitudinous and complicated affairs as head of a great family and owner of many *shōen* as well.

In cases where a great temple or shrine was the legal *Ryōshu*, statues or relics of the *Butsu* (Buddhas) or *Kami* (Deities) worshipped at these places were often set up in the *Azukaridokoro* of the *shōen*. These offices then became *matsuji* (subsidiary temples), or *massha* (subsidiary shrines).

Above the *Ryōshu* was the *Honke* (Legal Guardian of Manors). This post was held either by a *Kugyō*, or a member of the Imperial Family, an influential lady in the *Kōkyū*, or by a powerful temple or shrine with strong court connections. It was the duty of the *Honke* to persuade the *Dajōkan* to proclaim the *shōen* a *chokumenshō*, and to threaten the *Kokushi* with dismissal from office and punishments if they dared invade the *shōen* under this guardian's protection. In return, the *Ryōshu* sent the *Honke* a fixed yearly income previously agreed upon. If the *Honke* was successful in obtaining further rights or privileges for the *shōen*, his income was

increased; but if he failed to guard the *shōen* from adverse legislation and the attacks of the *Kokushi*, the *Ryōshu* generally broke his connections with such an unsatisfactory protector and found a more efficient *Honke* for his estates. Sometimes members of the Imperial Family, or *Sesshō* (Regents) and *Kampaku* (Civil Dictators) of the great Fujiwara family would grant themselves large tracts of the public domain, and then after reserving for themselves the post of *Honke*, distribute these lands among *Ryōshu* so as to free themselves from the burden of administrating their estates. In such cases, of course, the *Ryōshu* was acting illegally if he broke his connections with the *Honke* and placed the lands under the protection of another. It can be seen at a glance, however, that the key position in the whole *shōen* system was not held by the *Honke*, but by the *Ryōshu*.

Despite the fact that many disputes were settled by appeals to force, and that the tendency to do so steadily increased, the *Heian Era* was one in which a written land document carried a remarkable amount of weight. The *yoriudo, Jinushi* or *Tato, Geshi* or *Shōshi, Chūshi, Jōshi, Ryōshu,* and *Honke* all possessed documents guaranteeing them rights of office and rights in the land. These were generally perpetual rights, inheritable by women as well as by men, transferable with the permission of the *Ryōshu,* and capable of being divided and subdivided almost indefinitely. They practically constituted a form of paper currency. Hundreds have been preserved to the present day, and it is from these that a detailed description of the *shōen* system can be made.

STUDIES IN ORIENTAL ARCHAEOLOGY

I. Plano-convex Bricks

Valentin Müller
Bryn Mawr College

Delougaz has recently summarized our knowledge about plano-convex bricks and has advanced an hypothesis concerning their origin.[1] If I understand him correctly, he even offers two hypotheses which, however, are mutually exclusive. He says, p. 25: "Their peculiar form did not come about through any architectural or technical need, but merely as a result of rather primitive craftsmanship"; and, p. 37: "the peculiar form resulted from the brickmaking technique." He has, indeed, shown beyond doubt that the rectangular lower part got its shape from the wooden frame into which the lump of clay was laid and that the upper bulge is the surplus of the rolled lump not taken off. But why was this surplus taken off in the preceding period and left in the "plano-convex" period? The answer is obvious: the brickmaker wanted to retain this shape. Not the "technique" or "craftsmanship" is responsible for it, but the peculiar ideas of the architect who made some special use of the shape. I therefore think that Mr. Delougaz' second hypothesis is preferable, namely, that the plano-convex form imitates stone flakes for which such a shape is natural. This is the opinion also of other archaeologists, especially of Dr. Speiser.[2] Dr. Speiser argues clearly and irrefutably as follows: Since plano-convex bricks replaced the much more practical former type of wholly rectangular bricks, and since the queer plano-convex shape

[1] P. Delougaz. 1. Plano-convex Bricks and the Methods of their Employment. 2. The Treatment of Clay Tablets in the Field. The Oriental Institute of the University of Chicago. *Studies in Ancient Oriental Civilization*, Nr. 7, 1933.

[2] *AJA* XXXVII, 1933, 461. Also Jordan, 2 u. 3 Berichte über die Ausgrabungen in Uruk-Warka = *Abh. Preuss. Akad. Phil.-Hist. Kl.*, 1931, Nr. 4, 18; 1932, Nr. 2, 37, and E. Heinrich (*Schilf und Lehm*, Berlin, 1934, 52 f.) hold "that the plano-convex bricks were introduced by newcomers from a country in which stone was abundant." Doubts are expressed by Woolley (*Ur Excavations* II, 229 ff.) and Mallowan and Rose, *Iraq*, 2, 1935, 16.

can be understood only as an imitation of stones of such form, the people who first used the plano-convex bricks were conquerors coming from a country outside of Mesopotamia.

Delougaz supports his theory that the plano-convex bricks imitate stone by other arguments.[3] As the shape, so also the arrangement in "herringbone" pattern is natural for stones of this type and is actually found in various places in our times, namely around Mosul, in the Anti-Lebanon and in Southern France. There are also other features of architecture during the plano-convex period which point to an origin in a hilly country, such as columns, round fortifications, and others. Delougaz' ideas are perfectly sound and can be corroborated by further evidence. The writer of the present article hopes to bring forth this evidence soon and wishes here to deal with the herringbone pattern only.

There are some instances of the herringbone pattern occurring in the ancient world which have not yet been connected with the Mesopotamian ones, so far as the writer's knowledge goes. Furthermore, not only the pattern, but also the stone, which is the original material for it, is used. The places are Troy, where it is common in the first settlement with an isolated example in the fifth,[4] the contemporary settlement at Thermi on Lesbos,[5] and Eleusis in Attica, the latter also being of about the same date.[6]

Is there connection between Mesopotamia and the western places? It is true that the natural shape of the stone material suggests very strongly the technique of putting the flakes obliquely so that a spontaneous origin at different places is possible. Furthermore, it cannot be excluded with certainty that the modern use of the herringbone pattern around Mosul and in the Anti-Lebanon might be a survival from ancient times, but it is hardly possible to connect the occurrence in Southern France of today with specimens from Roman times[7] and the latter with the ancient Aegaean ones. The matter is, however, different in regard to the Aegaean and the Mesopotamian examples. First, they belong to the same time, because we can say, in spite of uncertainties and divergences of

[3] *Loc. cit.*, 25 ff.
[4] Dörpfeld, *Troja und Ilion*, I, 47; Blegen, *AJA* XXXVIII, 1934, 225.
[5] W. Lamb, *Annual Brit. School Athens*, 30, 1928/9, 1929/30, 6.
[6] Mylonas, *AJA* XXXVIII, 1934, 225, 260.
[7] J. Durm, *Baukunst der Römer*, 2 ed., 212.

opinion, that a date at about 3000 shows them in use both in Mesopotamia and in northwestern Anatolia.[8]

Then, connections between Anatolia and Mesopotamia can be proved for these early times. Hutchinson may be correct in denying close connection between the Uruk-ware and western Anatolian pottery;[9] but that the Uruk-ware is related to Anatolian ware in general and that the pottery from Troy and Yortan belongs to this group must be accepted as certain.[10] Many parallels between Troy and Mesopotamia, enumerated by Hall and Childe,[11] as in the shapes of daggers, pins, jewelry and so forth, do not appear before the second settlement, it is true, and also the well-known female figure from Troy which shows Mesopotamian influence[12] is later, but the ribbed sherds from Thermi[13] indicating metal prototypes remind one of the fluted silver vases from the Royal Tombs at Ur dating from the "plano-convex" period.[14] An independent origin of the herringbone pattern in Mesopotamia and in Anatolia thus becomes unlikely.

A second problem is the direction in which the connection between Mesopotamia and Anatolia took place. As to Anatolia, there is no doubt about the way the influence came. Western influence from Greece, where the pattern also occurs, as we mentioned, is excluded, not because the Greek example is later, but because the spread of civilization from Anatolia into Greece has been proved beyond doubt.[15] An origin in western Anatolia is hardly likely,

[8] For the Mesopotamian chronology see Frankfort, *Archaeology and the Sumerian Problem*, Chicago, 1932, 5 ff.; Herzfeld, *Arch. Mitt. Iran*, 5, 1933, 1 ff.; Albright, *AJA* XXXVIII, 1934, 608 f. For the chronology of Troy and Lesbos see H. R. Hall, *Civilization of Greece in the Bronze Age*, Chronol. Table; Reisner, *Antiquity* 1931, 200 ff.; Lamb, *loc. cit.*, 27. Åberg's attempt to lower the dates must be rejected, cf. Childe, *Antiquity* 1934, 363. Cf. also Frankfort, *Proc. I. Int. Congress of Prehistoric a. Protohist. Sciences*, 169.

[9] *Iraq*, 2, 1935, 211 ff.

[10] Frankfort, *loc. cit.*

[11] Hall, *loc. cit.*, 54; Childe, *The Aryans*, 132, and *Mitt. Anthrop. Ges.*, Wien, 63, 1933, 221.

[12] V. Müller, *Frühe Plastik in Griechenland und Vorderasien*, Augsburg, 1929, 22.

[13] Lamb, *loc. cit.*, 19 ff.

[14] Woolley, *loc. cit.*, pls. 160-2, 170.

[15] Frankfort, *loc. cit.*, 52 ff. and *Studies in Early Pottery of the Near East II*, 98 ff.; Haley and Blegen, *AJA* XIX, 1928, 334 ff., and F. Matz, *Zeitschr. f. Ethnol.* 66, 1934, 426.

for this region was at that time not a center from which influences spread eastward. Eastern connections of Troy I are, on the other hand, ascertained by the presence of copper,[16] because the origin of metallurgy must be localized with great probability in the eastern highlands.[17] With these highlands, however, the Mesopotamia of the Early Dynastic period also had connection, as the many similarities between Mesopotamian metal objects and those of the Caucasus region, such as socketed axes and adzes, eyelet and spatula pins, "Spanish" combs, and others, show.[18] We are, therefore, justified in surmising a connection for other features also, that is in our case the herringbone pattern and the shape of the material used. Whereas it is a matter of dispute whether the types of the early metal objects originated in Mesopotamia and are importations in the Caucasus region or whether the opposite is true, we can safely assume that the shape of the plano-convex bricks and their arrangement in the herringbone pattern is an importation into Mesopotamia. Delougaz thinks that it originated in the foothills rising from the Mesopotamian plain, but a more distant origin in the highlands themselves is made probable by its occurrence in the West.[19] A common source between western Anatolia and Mesopotamia is thus most likely.

I refrain from drawing any historical conclusion from this single point. Only after the connections of the other constituent elements of early Mesopotamian art have been discovered will it be safe to solve the ethnic problems.

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[16] Frankfort, *Studies* 2, 7; Lamb, *loc. cit.*, 35.

[17] Frankfort, *Studies* 2, 147 and *Archaeology*, 52 ff.; cf. H. Peake and H. J. Fleure, *The Corridors of Time* IV, Priests and Kings, 11 ff.; Speiser, *Excavations at Tepe Gawra I*, 167.

[18] Frankfort, 52 ff. Cf. Woolley, *loc. cit.*, 398 f.; Speiser, *loc. cit.*, 167.

[19] Speiser also is inclined to assume an origin not too close to Mesopotamia.

THREE RARE MANUSCRIPTS FROM THE JOHN FREDERICK LEWIS COLLECTION

MUHAMMED A. SIMSAR
UNIVERSITY OF PENNSYLVANIA

THE JOHN FREDERICK LEWIS Collection of Oriental manuscripts and miniature paintings was very carefully selected by the late Mr. Lewis during his lifetime, and after his death was presented to The Free Library of Philadelphia in honor of his memory by Mrs. John Frederick Lewis. It contains 35 Arabic, 52 Persian, 10 Turkish, 3 Hindustani, 8 Sanskrit, 1 Nepalese, 4 Pāli, 1 Siamese, 11 Armenian, 1 Coptic, 13 Ethiopic, 10 Hebrew, 1 Samaritan, and 2 Syriac manuscripts, and 1 Egyptian papyrus.

The Lewis Collection is particularly rich from the standpoint of its manuscripts of historical value. I could not possibly attempt to enumerate and to comment upon the various manuscripts dealing with the history of Iran, India, and Turkey, most of which were written by contemporary historians of their times and formed part of the libraries of Oriental kings, as shown by their seals and memoranda. Nor shall I attempt in this paper to mention all of these by their titles or names. But I shall describe three important manuscripts in connection with which I have made special studies while preparing the catalogue of the collection.

I

One of these (Fig. 1) which concerns itself with Turkish history, is a remarkably valuable manuscript, the "Last Will and Testament of Aḥmad Pāshā," the Grand Vizier of Sulṭān Bāyezīd II, who reigned from 1481 to 1512, and of his successor Sulṭān Salīm I, who reigned from 1512 to 1520. Its binding is missing, its size is 10 inches by 6¼ inches, and it consists of 33 folios written in a large beautiful Thuluth hand, 7 lines to a page inclosed by gold-rulings. The name of the scribe is Muḥammad and the date given is 917 A. H. (1511 A. D.).

In 1481 Sulṭān Muḥammad, the Conqueror of Constantinople, died. His son Bāyezīd was called from Asia Minor to take the rule. His younger brother Jem organized an army to march to the capital in an attempt to seize the throne. But Bāyezīd arrived

Fig. 1. The colophon of the Last Will and Testament of the Grand Vizier Aḥmad Pāshā

first, assumed control and sent out an army to defeat his brother. Our Aḥmad Pāshā of the manuscript first comes into the pages of Ottoman history when he was made the commander of the army sent to subdue Jem. He tried to arrive at a peaceful settlement but failed. Bāyezīd, impatient, led reënforcements to aid Aḥmad Pāshā, and Jem, faced with this overwhelming force, fled to Egypt, where he was well received by the Memluk sultan.

Since the Egyptian sultan had befriended his brother, Bāyezīd organized a campaign against him, and Aḥmad Pāshā was placed in command of this expedition. He was taken prisoner, and Bāyezīd sent a second army against Egypt, which forced the Egyptians to come to terms and to release Aḥmad Pāshā. Aḥmad Pāshā then returned to Constantinople, and was made the commander of the fleet, which post he retained for five years. He was later appointed grand vizier, a position previously held during Bāyezīd's reign by fourteen others, thirteen of whom ended their careers by being either beheaded or imprisoned.

In 1497, a year after his appointment as grand vizier, war was declared against Venice, and he was deprived of office because the sultan feared he might be too sympathetic towards the Christians and his former relatives, as he had been a Christian before he became a Muslim. Later from 1503 to 1506 he was made grand vizier for the second time, in recognition of his services in connection with a peace treaty which he concluded with Hungary. In 1509 there was a great earthquake in Constantionple. The tremors continued intermittently for forty-five days, and the whole country was terrified. The sultan himself encamped in the garden of his palace. Walls of the city cracked and fell into pieces; 109 mosques, 1610 houses, and thousands of persons perished. Many villages in Asia Minor were destroyed. Dervīsh Muḥammad, a contemporary Turkish historian, mentions that Aḥmad Pāshā had come to the rescue of the poor and horror-stricken people by handing out money and providing food to the destitute.

Two years later, in 1511, he became the grand vizier for the third time, but he narrowly escaped being killed during the revolts of the Janissaries, who were claiming Salīm, the youngest son of the sultan, as their king and were demanding that Bāyezīd resign in favor of Salīm. Aḥmad Pāshā was the only one to advise the sultan to resist this undignified demand. Bāyezīd followed Aḥmad Pāshā's advice,

but the army and the opposing party were strongly against him, and he finally resigned in favor of Salīm, who triumphantly entered the capital. Aḥmad Pāshā was naturally forced out of his office and was fortunate to escape with his life. Salīm's reign lasted until 1520. During this time there were two important campaigns, one against Iran and the other against Egypt. Salīm was victorious in both and Aḥmad Pāshā, whose ability had been recognized by the young sultan, was asked to accompany him in both these campaigns and received responsible commands. He was made grand vizier by Salīm, but resigned after a year, and later refused a second offer on the grounds of ill health. It was during his return from the Egyptian campaign in 1516 that he died, according to the German historian von Hammer, or was beheaded by the enraged sultan, according to Richard Knolles. Knolles wrote a history of Turkey in 1611, that is to say, about one hundred years after this event; the work seems to be authentic as it agrees with the writings of most of the contemporary Turkish historians. I shall quote both these authorities before ending this story. Knolles calls Aḥmad Pāshā "Cherseogles," evidently a corruption or mispronunciation of Hersek-Oglu, the name given to him by Turkish historians.

"But now that we have fallen into the remembrance of this Cherseogles, it shall not be amiss, both for the honour of the man, and the great love he always bore unto the Christians, to step a little out of the way, to see the cause why he being a Christian born, turned Turk. For he was not (as almost all the rest of the great men about Bajazet were) of a Child taken from his Christian Parents, and so brought up in the Mahometan Religion; but now being a man grown, turned Turk; yet so, as that he never in heart forgot either the Christian Religion or love towards the Christians; a thing not common among such Renegates. He being the Son of one Cherseogles, a small Prince of Illyria near unto the Black Mountain; and going to be married unto a Lady whom he most entirely loved, and unto whom he was already betrothed, honourably descended of the House of the Despot of Servia; his intemperate Father with lustful Eye beholding the young Lady of rare Feature and incomparable Beauty, desired to have her for himself; and regarding more the satisfying of his own inordinate desire, than his own honour, or of the Fatherly Love of his Son, took her in marriage himself; all his Friends laboring in vain to dissuade him, and with open

mouth crying shame of so foul a Fact, wherefore the young man, moved with the dignity of so great an injury, and driven headlong with despair, fled first to the Turks Garrisons which lay not far off, and from thence to Constantinople; where the fortune of the man was to be wondered at; For being brought before Bajazet, who with cheerful countenance entertained him, for that he was honourably descended, and well liked both of the man, and of the cause of his revolt; smiling upon him, said, 'Be of good cheer Noble Youth, for thy great courage is worthy of far greater fortune than thy fathers House can afford thee; now in stead of the Love wrongly taken from thee by thy Father, thou shalt have given thee in marriage the Daughter of a great Emperor, of rare and singular perfection.' And long after abjuring his Religion, and changing his name of Stephen to Achomates (Aḥmad) and Cherseogles, (Hersek-Oglu) he married one of Bajazet his Daughters, a Princess of great Beauty; and deserved to have a place amongst the Bassas (Pāshās) of great honour in the Court."[1]

Von Hammer, however, quoting Awliyā Chalabī, the Turkish historian, speaking of the return from Egypt says:

". . . et non loin d'Alep mourut le vesir Herseck-Ahmed-Pascha, fils de duc de Saba Etienne-Cossarich, qui, pendant un demi-siècle, avait rendu tant d'importants services à l'empire ottoman, et avait occupé quatre fois la première dignité de l'État. Sa mémoire est conservée par la mosquée et la cuisine des pauvres subsistant encore aujourd'hui, qui furent construites à la pointe la plus extrême du golfe de Nicomédie, sur le rivage méridional."[2]

Both von Hammer and the manuscript refer to the important charitable works of Aḥmad Pāshā. The manuscript is his last will, and according to it he left all his wealth and huge estates to charity. The will directs the disposition of funds for the erection of mosques and the care and the feeding of the poor villagers. Whole villages belonged to him, mostly inherited from his wife, the sultan's daughter. These were situated both in Asiatic and in European Turkey, and some of them are still known by the names set forth

[1] Richard Knolles, *The Turkish History from the Origin of that Nation*, 1. 329, London, 1687.

[2] P. J. Hammer, *L'Histoire de L'Empire Ottoman*, French Translation by Dochez, 1. 457-458, Paris, 1844. For further particulars see Hammer, *Geschichte des Osmanischen Reiches*, 2. 264 ff., and 526-27, Pesth, 1827-1835.

in the will; the income from these is given to carry out the purposes of the will. Administrators of proven piety and integrity are named, and their salaries are fixed, and even the number of sheep to be slaughtered daily and loaves of bread to be baked for the poor are enumerated.

On the colophon of this manuscript, besides the name of the scribe and the date, the names of Sinān Pāshā, and Pīrī Pāshā, the two other grand viziers of the time, appear as witnesses. There are two other names which so far have not been identified.

II

The second manuscript (Figs. 2, 3) deals with the history of Iran. It is a document written by the order of Sulṭān Ḥusayn Ṣafavī, acknowledging a bequest in Isfahan. The volume is 9 inches by 6 inches and consists of thirteen folios by the famous calligrapher Muḥammad Bāqir al-Ḥusaynī. The first page and the binding are missing, but the document has been rebound in modern English red levant morocco.

Sulṭān Ḥusayn reigned from 1694 to 1721. In 1721 Maḥmūd, an Afghan chief, marched against him, defeated his army, and besieged the capital city Isfahan. Shah Ḥusayn's son Ṭahmāsp, in the meantime, deserted his father and escaped to the mountains of Māzandarān. The shah on account of famine offered to capitulate. Maḥmūd entered Isfahan in triumph, and seated himself on the throne in the royal palace. In 1723 Maḥmūd invited three hundred of the principal Persian nobility to a banquet and murdered them. To prevent their children's attempt at vengeance he also murdered all of them. Not content with this, in 1725 he assembled all the captive members of the royal family, except the shah and the queen, in the courtyard of the palace, and caused them all to be murdered. At length Maḥmūd died in that same year; he was succeeded by his cousin Ashraf. In 1729 Ṭahmāsp, the fugitive prince, assisted by Nādir Khān, who later became the shah, assembled an army and marched against Ashraf. Ashraf alarmed led an army against the prince, but was defeated and fled through the town, not forgetting to murder the poor old Sulṭān Ḥusayn. Shāh Ṭahmāsp entered triumphantly into Isfahan.

Now this manuscript in the Lewis Collection, which makes a be-

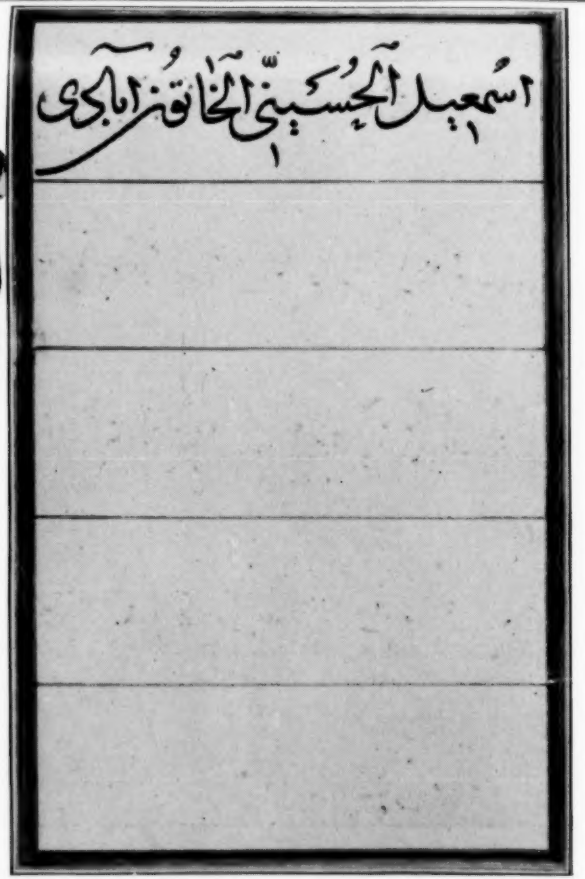

Fig. 3. Page from the Bequest of Sulṭān Ḥusayn Ṣafavī bearing seals of witnesses

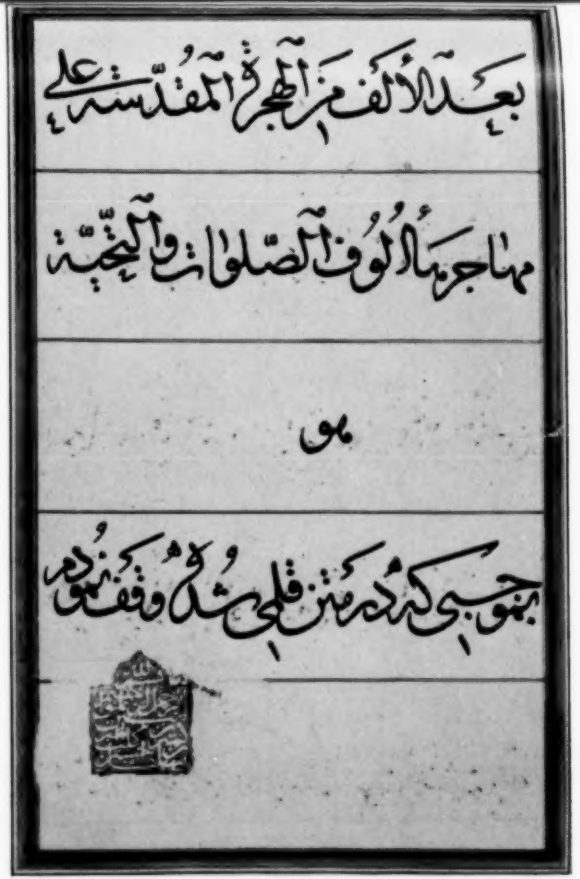

Fig. 2. Page from the Bequest of Sulṭān Ḥusayn Ṣafavī bearing his seal

Fig. 4. The front flyleaf of the manuscript bearing the autograph and the seal of Jān Sipār Ḵẖān

quest to the city of Isfahan by Sulṭān Ḥusayn, sets aside what, in effect, was a funeral parlor for the needy Muslims where they might wash and prepare the bodies of their dead for proper burial, in full accordance with the requirements of Islam. It is stipulated in the document that the building and its equipment may not change hands, may not be sold, and should be free to the poor forever.

Each page of the manuscript is gold-ruled and the sultan's name whenever mentioned appears in gold. Each page also bears the seal of one of the shah's sons and that of the Chief Justice, and of two other officials whose positions have not yet been identified. The date of the manuscript is 1118 A. H. (1706 A. D.), or twenty-three years before Sulṭān Ḥusayn's death.

III

The third manuscript (Fig. 4) concerns itself with northern India. It contains a poem in praise of 'Alī, the fourth caliph, by the Persian poet Mullā Ṭughrā who lived during the seventeenth century. It is 7 inches by 5 inches and consists of 12 folios of gold-sprinkled paper written in excellent Nasta'līq style. It bears the seal and the handwriting of Jān Sipār Khān, the Turkoman Amīr, a contemporary of the last of the Mughal emperors Aurangzīb. Above the large circular seal impression, in large Shikastah style, appear these words in Arabic: "From the most humble slave," and then the seal impression below it reads: "Jān Sipār Khān, Khānah-Zād-i- (a slave born in the house of) Shāh-i-'Ālam (which was the title of Muḥammad Mu'aẓẓam, the second son of Aurangzīb) Pādishāh-i-Ghāzī (the victorious king) 1121 A. H. (1709 A. D.) Sanah 3 (the third year of the reign of the emperor)." Underneath the seal a little to the right, written again in large characters appears the word "Hū" (pronounced as *hu*, but written *huwa*), which refers here to the God Almighty, and means "He (is) or He (exists)," but is more generally used as a substitute for the well-known Qur'ānic formula: "In the name of the Most Merciful God." Then follow four lines in Persian poorly written in small Indian Shikastah which read: "From the possessions of the humble slave Jān Sipār Khān, the son of Rustamdilkhān, the son of Jān Sipār Khān al-Ḥusayn al-Mukhtār al-Sabzawārī, may the Lord forgive his sins and cover his misdeeds. From the gifts of Nawwāb (prince) Ṣāḥib (sir, master) and Qiblah (another title) Nawwāb

Mukhtār Khān to whom may God manifest His proof. Sanah 1122 A. H. (1710 A. D.)."[3]

Jān Sipār Khān was the second son of Mukhtār Khān Sabzawārī, an Amīr during the reign of Aurangzīb. At the time of his death in 1701 he held the ṣūbahdārī, or the Governorship, of Hyderabad. The grandfather of Jān Sipār Khān, who bore the same name, was an Amīr with an army of four thousand men during the reign of Emperor Jahāngīr. He was appointed Governor of Allahabad in the first year of Shāh Jahān, 1628 A. D., and died there during the same year.[4]

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[3] The traditional phrase أَنَارَ اللّٰهُ بُرْهَانَهُ has been translated by Lane in his Arabic Iexicon as: "God taught him His proof." أَنَارَ, the fourth form of a hollow verb, is Active Perf. 3 s. m., and should therefore have been translated as: "has taught," or rather, "has enlightened." But in such a connection as that of our text this phrase is employed as a wish or a blessing upon the deceased.

[4] See *An Oriental Biographical Dictionary*, by T. W. Beale. Revised and enlarged by H. G. Keene, London, 1894.

SANSKRIT *parśu* AND *paraśu*

WILLIAM M. AUSTIN AND HENRY LEE SMITH, JR.
PRINCETON UNIVERSITY

FROM THE beginning of Vedic research there has existed confusion[1] between the two words *párśu* "rib," later also a sort of sacrificial sickle, and *paraśú* "ax," due largely to the similarity of the words themselves and to the fact that occasionally in later literature *paraśú* was, *metri causa*, actually contracted to *párśu*.[2] This confusion is evident only in Sanskrit, not in Indo-European, since *paraśú* is of course equivalent to the Greek πελεκύς, going back to an IE. **pelĕku*, borrowed from the Akkadian *pilaqqu* (from Sumerian *balag*). *Párśu* is IE. **perĕku*, whence Avestan *pərəsuš* "rib," "side," Ossetic *fars* "side," and Old Church Slavic *prŭsi* "breasts." This note is an attempt to distinguish and explain the two Sanskrit words once and for all.

Párśu occurs uncompounded in the Rig-Veda four times, twice (8. 6. 46 and 10. 86. 23) unquestionably as a proper name. In the latter passage "the daughter of Manu, Párśu (*i. e.* Rib) by name" suggests an interesting parallel to the Biblical story of the creation of Eve. Twice (in 1. 105. 8, repeated in 10. 33. 2) *párśavaḥ* means simply "ribs." The latter passage reads, "from all around ribs (*párśavaḥ*) give me pain like co-wives." Whether "ribs" here refer to the walls of the well in which Trita was confined, as Sāyaṇa and the Nirukta (4. 6) take it, or to the ribs of the sufferer himself need not concern us. Most modern commentators and translators favor the interpretation of a needy poet suffering from hunger.[3]

In the Atharva-Veda, *párśu* means simply "rib" in 9. 7. 6, 10. 9. 20, 11. 3. 12, but it occurs with the meaning "sickle" in

[1] As evidence of this confusion, cf. Sāyaṇa, below, and Macdonell and Keith's *Vedic Index of Names and Subjects*, I, 503, where *párśu* "sickle" is mistakenly explained as "being apparently a variant of *paraśú*."

[2] *Rāmāyaṇa* 3. 28. 24: *prāsān pāśāṅs tathā parśūn kuttāṅś ca kuṇapāṅs tathā*; cf. also *Trikāṇḍaśeṣa* 2. 8. 25, *Harivaṁśa* 5870, etc.

[3] Cf. Geldner *Der Rig-Veda* I, 124; Grassmann's translation II, 471; and especially Bloomfield, *Rig-Veda Repetitions*, p. 114. Ludwig erratically considers it (*Der Rig-Veda* II, 578) a proper name.

7. 28. 1 and 12. 3. 31. The latter passage might be rendered as follows: "Stretch forth the sickle (*párśu*) speedily, take (it) with haste; unharming let them cut the plants at the joint." In 7. 28. 1, when compared with the obviously parallel passage in the Taittirīya Saṁhitā 3. 2. 4, we find a striking example of the confusion, already mentioned, which grew up between *párśu* in its secondary meaning "sickle" and *paraśú* "ax." The AV. passage reads: *vedaḥ svastir drughaṇaḥ svastiḥ páraśur vediḥ paraśúr naḥ svasti*, "the sacred grass is well-being, the tree-slayer is well-being; the sickle is sacrificial altar, the ax is our well-being." The chiastic order here makes it evident that the peculiar form *páraśu* (with accent on the first syllable) is equated with *vedaḥ* (hence *párśu* "sickle" for cutting sacred grass) and *drughaṇaḥ* "tree-smiter" is equated with *paraśú* "ax." This interpretation is further verified, first, by the fact that the commentator understands for this *páraśu* the word *párśu* "sickle," and, second, by the above-mentioned passage from the Taittirīya Saṁhitā: *sphyaḥ svastir vighanaḥ svastiḥ párśur vediḥ paraśúr naḥ svastiḥ*.[4]

We have seen, then, that in the Rig-Veda *párśu* means only "rib," but that it came also to mean "sickle" in the Atharva-Veda. This semantic transfer becomes instantly clear when we take into consideration the rib-like shape of the instrument and realize that the rib of a horse was actually sharpened to make a sickle.[5]

There remains, however, a compound of *párśu* in the Rig-Veda which has been consistently misinterpreted since Sāyaṇa. In 7. 83. 1, a hymn addressed to Mitra and Varuna by the priests of the victorious king Sudās, we read: *yuvāṃ narā paśyamānāsa āpyam prācā gavyantaḥ pṛthuparśavo yayuḥ*. We translate this: "Beholding you and your friendship, oh men (Mitra and Varuna), they went forward, eager for booty, broad-chested." Sāyaṇa comments on *pṛthuparśavaḥ* as follows: *pṛthuparśavaḥ pṛthur vistīrṇaḥ parśuḥ parśvāsthi yeṣāṁ te tathoktāḥ vistīrnāśvaparśuhastāḥ*, that is: "*pṛthuparśavaḥ: pṛthu* 'broad,' *párśu* 'rib-bone'; so spoken

[4] *vighanaḥ* "wooden pestle" is here substituted for *drughaṇaḥ*, since the passage deals with the Soma sacrifice; cf. Bloomfield *ZDMG* 48. 546. On both passages cf. the Whitney translation of Atharva-Veda, pp. 407, 408, and Keith's *The Veda of the Black Yajus School*, I, 242.

[5] Cf. Sāyaṇa, quoted below, and Nirukta 4. 3 where the "side" (*pārśvam*) is spoken of as "shaped like a sickle" (*parśu-mayam*).

of because they had the broad ribs of horses in their hands." Among those who favor Sāyaṇa's view should be noted particularly Böhtlingk and Roth ("breite Krummsäbel, oder Hippen tragend")[6] and the translations of Wilson, Grassmann, and Hillebrandt. But since this is a battle hymn, we are confronted with the obvious difficulty of arming our heroes with agricultural or sacrificial implements. Realizing this, Grassmann translates the compound "mit breiten Säbeln," while Zimmer, Geldner, and Griffith prefer a translation "with broad axes."[7] This latter interpretation is untenable since, in the first place, *paraśú* "ax" is apparently not contracted to *párśu* until epic times, and, secondly, because in the Rig-Veda *paraśú* is used only to designate the ax of the wood-cutter. For example, in 7. 104. 21 we read "like an ax, Śakra, that splits the timber," and again in 10. 28. 8, "the gods came bearing axes and cutting the woods with their followers."[8]

Ludwig again differs by translating *pṛthuparśavaḥ* as a dvandva compound, "the Pṛthus and the Parśus," and the same view is held by Weber.[9] That the subjects of this Vedic hymn should be extraneous tribes, whether Parthians, Persians, or the Parśus known to Pāṇini, is impossible and this view has been adequately refuted by Zimmer.[10]

The translation which has already been suggested for *pṛthuparśavaḥ*, namely "large-ribbed," "broad-chested," "strong"[11]

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[6] The curved sword, or scimitar, was apparently unknown in India before epic times.

[7] Grassmann, *Rig-Veda*, II, 365; Zimmer, *Alt-Indisches Leben*, p. 134 ff.; Geldner, *Siebenzig Lieder des Rig-Veda*, p. 32, and Wilson, *Hymns of the Rig-Veda*, II, 79.

[8] Cf. also RV. 1. 127. 3, 6. 3. 4, 9. 67. 30, 10. 43. 9, 3. 53. 22, 4. 6. 8, 10. 53. 9, 1. 130. 4, 5. 48. 4.

[9] Ludwig, *op. cit.*, V, 547 and Weber, *Indische Studien*, IV, 379.

[10] *Op. cit.*, pp. 433, 434.

[11] We had already reached this conclusion when we found that Macdonnell and Keith in their *Vedic Index of Names and Subjects*, I, 504, suggest this as a possibility on the authority of Roth following Sāyaṇa (reference not given for either). We have seen above Sāyaṇa's exegesis, and Roth nowhere suggests "with large ribs." Apparently Macdonnell and Keith misread the St. Petersburg Lexicon where a few lines above the entry *pṛthuparśu* Roth has "*pṛthupakṣas*, nach Sâj. adj. 'breite Seiten habend,'" used for sides of a wagon in RV. 8. 26. 23. We later noted that Geldner in *Rig-Veda in Auswahl*, I (Glossar) renders *pṛthuparśu*, "mit weiter, ge-

appears now as the only true meaning after an examination of the history of *párśu* and *paraśú*. The compound corresponds to a host of others, such as *pṛthu-vakṣaḥ* "broad-chested," *pṛthu-vaṁsa* "broad-shouldered," and *pṛthu-pāṇi* "broad-handed," *i. e.* "generous." Compare also Lithuanian *piršingas* (Geitler) "breitbrüstig," from *piršas* "breast" (cf. Old Church Slavic *prŭsi*, above).

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hobener Brust," but this has borne no fruit since it has not appeared in any translation, by him or any one else.

BRIEF COMMUNICATIONS

The Illuministic Sufis

That Sufism is a way rather than a set of disciplines and doctrines has now been established.[1] Yet the fact that the early Moslem mystics sought support for their views in the Koran[2] can hardly annul the concept that theirs was a psychological behavior engendered as a direct response to the all but human aspiration for personal approach to the Deity and a spiritual contact with religious truth. In Arabic literature the form *ṣūfi* first appears in the middle of the ninth century when it was applied to a certain class of ascetics. But Moslem ascetics must have lived before that time.

Syria in pre-Islamic days saw the rise of the three-staged mystical experience—Purgation-Illumination-Perfection. For in the sixth century Dionysius, a Syrian monk, developed a mystical theosophy based on Hellenistic sources in the main.[3] But the first mystics of Islam, it must be conceded, were only interested in the first of these three stages, that is, Purgation. Gradually, however, they began to develop the second stage, that is, Illumination. Al-Muḥāsibi[4] who pioneered with his disciples in the pathways of Purgation was one of the first to declare that as purification brings freedom from the attachments of this world one might expect to attain the stage of Illumination and thence proceed to the unitive life with God.

Ibn-Masarrah of Cordova (A. D. 883-931) founded the Illuminis-

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[1] D. B. Macdonald, "The Unity of the Mystical Experience in Islam and Christendom," in *The Moslem World*, 25. No. 4 (Oct. 1935). 325-335.

[2] Such passages as Koran 4: 96, 9: 113, 33: 47 must have served them in good stead. When Sufism was passing through a crisis and especially after the execution of al-Ḥallāj (A. D. 992) was in the danger of being outlawed, the deeply concerned Sufis sought to make their system conformable to the strictest standards of orthodoxy. See A. J. Arberry, *The Doctrine of the Sufis*, Cambridge, 1935, pp. xiv, xv.

[3] Margaret Smith, *Early Myticism in the Near and Middle East*, London, 1931, p. 79.

[4] Abu-Abdullāh Ḥārith ibn-Asad (A. D. 781-857), was born at al-Baṣrah and taught in Baghdad. See Margaret Smith, *An Early Mystic of Baghdad*, London, 1936. Professor Massignon describes him as "the true master of primitive Islamic mysticism."

tic (*Ishrāqi*) and pseudo-Empedoclean school.[5] Later on an essential element of the Ishrāqi teaching—the metaphysical doctrine of light—reappears in the Divine Comedy.[6] Here we find, reproduced a century after ibn-'Arabi,[7] most of the pictures the latter used of the realms beyond the grave. Seen in this perspective Dante would pose as one of the ardent adherents of the Illuministic school. In the main, it may here be stated, the doctrine of the Illuministic Sufis was based on a spiritual philosophy with a mystical theory of knowledge. God and the world of light were interpreted as radiation and our process of cognition as illumination from above.[8] The system was consummately referred to as *ḥikmat al-ishrāq* (wisdom of Illumination), Abu-al-Futūḥ al-Suhrawardi[9] and his colleagues being largely associated with it. They were undoubtedly steeped in the syncretic philosophy of Hellenism which reached the Hither Orient in the form of neo-Platonic, Hermetic,[10] and allied sources, and was there woven with Persian and other speculations.

A manuscript in the Bārūdi Collection, acquired by Mr. Robert Garrett of Baltimore in 1925,[11] entitled *Risālat Qawānīn Ḥikam al-Ishrāq ila Kull al-Ṣūfīyah bi Jamī' al-Āfāq* (A Treatise on the Articles of the Maxims of Illumination addressed to all the Mystics of the World), by abu-al-Mawāhib al-Shādhili (died A. D. 1477/8), harks back to the works of al-Suhrawardi and ibn-'Arabi. The author belonged to the Shādhili fraternity of Sufis, founded by

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[5] Miguél Asin Palacios, *Islam and the Divine Comedy*, tr. Harold Sunderland, London, 1926, p. 264; while no great champion of al-Ishrāq is recognized in the Iberian Peninsula before ibn-Masarrah it might still be more accurate to accord him the honour of being the founder only in Spain of this movement.

[6] *Ibid.*, pp. 164-165; also consult the same author's *Abenmasarra y su escuela*, Madrid, 1914, pp. 120-121.

[7] Abu-Bakr Muḥammad ibn-'Ali Muḥyi al-Dīn al-Ḥātimi al-Ṭā'i, born at Murcia A. D. 1165, died in Damascus 1240.

[8] Hermes, Agathodaemon, Empedocles, Pythagoras, and Plato were prominent authorities often described as prophets or inspired sages.

[9] Aḥmad ibn-Ḥabash ibn-Amirāk Shihāb al-Dīn, A. D. 1154-1191; born at Suhraward, Persia, lived and studied at Marāghah, Iṣfahān, Baghdād, and finally Aleppo, where he was murdered.

[10] Consult T. J. de Boer, "Urāni," in *ZA* 27 (1912). 8-15.

[11] Now deposited at Princeton University Library. A Catalogue of the entire Garrett Collection of Arabic MSS is in press.

abu- al- Ḥasan 'Ali ibn-'Abd al-Jabbār al-Shādhili [12] who was born at al-Shādhilah near Jabal Zafrān in Tunisia.[13] It would seem that he had based his doctrines on a foundation introduced into Morocco and North Africa by the Sevillian teacher of ibn-'Arabi, the so-called abu-Madyan. Such men as 'Abd al-Salām ibn-Mashīsh, who was the master of the founder of al-Shādhilīyah, as well as 'Abd al-'Abbās al-Mursi and ibn-'Abbād of Ronda are great Shādhili thinkers who became legitimate heirs of the Illuministic trends of ibn-'Arabi.

The author of the MS referred to above lived in Cairo during the last century of the Mamlūk period. His work represents one of the very few treatises written by Sufis on this fascinating phase of Moslem mystical life. It opens two vistas before our vision: the one leading into the depth and mystery of the Illuministic school of thought, the other disclosing the birth and rise of Moslem Sufi fraternities of which al-Shādhilīyah—the author's own order—is a unique organization whose influence was unexcelled in the annals of Moslem mysticism.

EDWARD J. JURJI.

The Institute for Advanced Study,
Princeton, N. J.

The Parts of a Vīṇā

I have discussed the terminology of the parts of the old Indian postless harp in JAOS 50. 244, and 51. 47 and 284.[1] Jaiminīya Brāhmaṇa II. 69-70 affords additional information. As the text and German version by Caland, *Das Jaiminīya Brāhmaṇa in Auswahl* (Amsterdam, 1919), p. 143, are not very accessible, an English rendering, with brief comment, is given here:

"The Progenitor (Prajāpati) and Death (Mṛtyu) were sacrificing, in opposition to one another. The sacrificial utensils were those that are now used in Iṣṭi sacrifices. What was sung as the laud (i. e. by the Udgātṛ), or recited as formulae (i. e. by the Hotṛ) or

[12] Died A. H. 656/A. D. 1258.

[13] 'Afīf-al-Dīn al-Yāfi'i, *Kitāb Mir'āt al-Janān*, Cairo, A. H. 1339, vol. IV, p. 146.

[1] See also "A passage on *vīṇā*-playing," *Zeit. für Vergleichende Musikwissenschaft*, III, p. 88, 1935 (1936).

done (i. e. by the Adhvaryu), that was the Progenitor's party. But what was sung to the harp, or enacted (*nṛtyate* 'danced'), and done, by way of mere entertainment (*vṛthā*), that was Death's party.[2] These two parties were now equally matched; one was as great as the other. For a long time, for many years, neither got the upper hand. Then the Progenitor willed, 'May I overcome Death!' He saw the perfect-form (*sampadam*) and due-measure (*saṃkhyānam*) of the sacrifice;[3] therewith he overcame Death. When he (Death) was overcome by these, he grieved; falling down hitherward, he entered the gynaeceum (*patnīśālām*), wives (*patnīḥ*) accordingly. . . .[4] What had been his lauds, his recitative, are now what people sing to the harp, or enact, or do to please themselves (*vṛthā*); his sacrificial stake (*yūpa*) is now the neck (*daṇḍa*) of the harp; his tie-ropes (*raśanā*) are now the bindings (*upavāṇa*) of the harp; his soma-vessel (*droṇa-kalaśa*) is now the basket (*sūnā*)

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[2] It is, of course, *vṛthā*, not *nṛtyate*, that carries the pejorative implication. Without reopening here the whole problem of "Mysterium und Mimus in Ṛgveda," it may be observed that RV. X. 72. 6 *nṛtyatām iva* affords an authority for "dancing" in the ritual, which it is well known is modelled "on what was done by the Devas in the beginning," and that Kauṣītaki Brāhmaṇa, XVII. 8 speaks of the sacrificing priests as "dancing," or rather as we should understand, "enacting" (*ninartayanti*).

[3] JB. II. 73 explains that the *saṅkhyānam* is what is immortal (*amṛtam*) in the sacrifice, the *asaṅkhyānam* what is deathly, or mortal (*martyam*). *Saṅkhyānam* is apparently tantamount to *pramitam* and might be rendered by "ordinary" (n. as in "Ordinary of the Mass"), with "inordinate" or "irregular" as antithesis. As in BG. there is no reference to the philosophical system. *Sāṅkhya* in BG. would appear to be the "way of sacrifice" as distinguished from the "way of works." If *sampad* and *saṅkhyāna* are the "perfection and due measure" of the sacrifice, it is easy to see how *sāṅkhya* may have meant "one expert in sacrifice," or "one who performs the sacrifice aright, or correctly"; and it may be in this sense that the Vedic *ṛṣi* Atri is *sāṅkhya* in the *Anukramaṇī*.

[4] The rest of the sentence I cannot understand, but the sense is evidently that the music of Death became the pastime of women. Cf. ŚB III. 2. 4, where the mundane Devas, designing to please her (*pramodayiṣyāmahai*) succeed in seducing Vāc, the Gāyatrī, by means of the music of harp and voice; from the Gandharvas' lauds and praises she turns away to the dancing and singing (*nṛttaṃ gītam*) of the Devas; "and it is because Vāc thus turned, that right here and now (*itar-hi*) women are given to vanities (*mogha-saṃhitaḥ*), for it was thus that Vāc turned thereto, and other women do as she did. And hence it is that to one who dances and sings they take a fancy most readily."

of the harp; his soma-pressing skin (*adhiṣavaṇa carman*) is now the parchment (*carman*) of the harp; his perforations (*uparava*) are now the openings (*ākāśa*) of the harp; his pestles (*grāvan*) are now the plectra (*vādana*) of the harp; his seven metres (*chandas*) with fourfold increment [5] are now the strings (*tantrī*) of the harp; his ten-nights lauds (*daśarātra*) are now the ten fingers (*daśānguli*). It was because the sacrifice was twofold that he came to grief; the sacrifice is really one, the sacrifice is just the Progenitor himself." [6]

The analogies enable us to clear up a few points that have remained obscure. For *upavāṇa* we suggest the emendation *upavīṇa*, or at any rate, that *upavāṇa* and *upavīṇa* are synonymous; the analogy makes it evident that the reference is to some kind of "bindings," and we suppose this to be the binding at the junction of the neck and belly, plainly shown in many of the representations. *Sūnā* is seen to be synonymous with Skr. *ambhaṇa* or *bhāṇḍa*, and Pali *doṇī*, *doṇikā*; [7] and *ākāśa* with *chidra*. *Chandas* appears to be synonymous with *svara*. Caland (*l. c.*, p. 144) may be right in thinking that the "ten fingers" are simply the ten fingers, all of which are obviously made use of in one way or another in playing; in which case *angulinigraha* may mean, in quite the modern sense, "fingering."

ANANDA K. COOMARASWAMY.

Museum of Fine Arts, Boston.

[5] For the meaning of this see ŚB X. 3. 1. 1.

[6] I cannot see why Caland says that the last sentence is "nicht deutlich."

[7] *Sūnā*, "basket," may imply that the belly of the harp could be made of basket-work rather than wood.

REVIEWS OF BOOKS

Subartu. Beiträge zur Kulturgeschichte und Völkerkunde Vorderasiens. Von ARTHUR UNGNAD. Berlin und Leipzig: WALTER DE GRUYTER & Co., 1936. xi + 204 pages. RM 10.

Ever since the Amarna letters became known, historians have been aware of the fact that there existed in the middle of the IInd millennium B. C. in Upper Mesopotamia a state of great power, called the kingdom of Mitanni. Its king, in his correspondence with the pharaoh, employed a language which, though written in the usual cuneiform characters of the period, proves to be individual. Certain words of this language recur in vocabularies and are attributed by their ancient compilators to the language of Subartu.

The fortunate discovery of the Hittite archives has contributed to our knowledge also of this language and this nation; there the respective people are called Hurrites. This term is used also in the Amarna letter which is composed in the "Subartu" language and may, therefore, be taken as a native designation.

Archaeology has so far to only a relatively small extent devoted itself to the Hurritic problem. The excavations of Baron von Oppenheim at Tell Halaf are undoubtedly its main contribution. They have produced particularly interesting sculptures and a large amount of painted pottery.

The interpretation of the complex facts has already caused much discussion.[1] Ungnad's book is an original and important contribution to the subject. The author sets forth the thesis that the culture of Tell Halaf, characterized by the sculptures and the painted pottery, belongs to the Subareans whom he considers the basic population of Upper Mesopotamia since primitive times. He, then, shares

[1] The author announces in his preface (p. v) that he could not take into consideration the pertinent publications which have appeared since 1933. This is very much to be regretted, particularly for the reason that with Herzfeld's, *The Tell Halaf* (1934) an exception has been made. Remarks like those on p. 187 (middle of the page) are out of place because A. Moortgat in *Die bildende Kunst des Alten Orients und die Bergvölker* and the reviewer in *Hethiter, Churriter und Assyrer* have treated the subject in 1933 and 1936 respectively.

and defends the opinion of the excavator of Tell Halaf, which has been eloquently advocated also by E. Herzfeld; he also opposes and wants to refute all those who make the Hurrites (Subareans) enter Mesopotamia not before the 20th century B. C.

The book is divided into three parts. An introduction deals with the racial history of the Near East. The author stresses the fact that in Upper Mesopotamia a race dominates which is characterized by a flat occiput ("Steilschädel"). Its territory in the Near East coincides with that covered by painted ceramics ("Buntkeramik"). The two features, according to Ungnad, are to be coördinated. A convenient term for this whole territory is looked for and found in the Subartu of the Akkadian inscriptions.

The second part of the book, therefore, collects all references to the country Subartu and its inhabitants, the Subareans, from the cuneiform documents. It gives full information also on the extant linguistic evidence such as glosses and proper names. This part of the book is of value regardless of the attitude that may be taken towards the author's main thesis.

The third chapter turns to the interpretation of the collected evidence. It reaches the conclusion that Subartu, attested since the Akkad dynasty (middle of the IIIrd millennium), extended from the Zagros and Western Persia in the East to Syria and the Mediterranean coast in the West; the Hurrite confederacy of the IInd millennium, placed in the foreground by other scholars, is merely an episode in the long and significant history of the Subareans. The parallelism between the philological evidence and the archaeological and anthropological facts, in Ungnad's opinion, is so close that their coördination is not only justified but imperative.

The reviewer's criticisms can be limited to the first and the third chapters.

The first chapter contains a good deal of mysticism. The principle that race, language, and culture are to be coördinated is highly disputable. No matter whether it can be applied in considering "primitive" times, it is certain that the condition in the Near East, during historic as well as prehistoric times, is far from "primitive." The variety of ethnic names—I mention the Khattians, Lullubeans, Guteans, Kassites and Elamites; and how many may since have been entirely forgotten?—suggests a very complex situation which seems unduly simplified by the preponderance which Ungnad assigns to the Hurrites-Subareans. Furthermore we must

not draw hasty inferences from the ethnic situation of today concerning conditions in the IVth millennium B. C. Such a procedure involves a large margin of error, which in my opinion makes it practically valueless. And even if the existence of the "Steilschädel" in Mesopotamia of the IVth millennium is admitted, how can we know that this race was primarily Subarean? Why not pre-Subarean? If the type persisted in spite of all ethnic movement, it cannot give any indication whatsoever of an ethnic significance.

The "Buntkeramik" is treated as one of the achievements of the Hurrites-Subareans. Before it is used for historical conclusions, it should be properly located and dated. Ungnad takes for granted, first, that it covers all of Upper Mesopotamia; second, that it exists throughout the entire history of its mounds. Both these suppositions are wrong; in fairness to the author it must be added that this became clear only after the book had left his hand.

The investigations of M. E. L. Mallowan in the Khabur valley have furnished the invaluable information that the so-called Tell Halaf ware ceases to occur only a little south of the Tell Halaf and seems to have been introduced from a more northern region.[2] The valley of the lower Khabur is free from "Buntkeramik," and, we may add, none is known from the Euphrates valley.

The stratification of the Tell Halaf has not yet been systematically investigated. The evidence lacking must be supplemented from other sites. It is known now from Arpachiyah,[3] that the Tell Halaf ware precedes the 'Ubaid stage of the culture of Lower Mesopotamia; from Chagar Bazar,[4] a site not far from the Tell Halaf, that it is Chalcolithic and is followed at this site, at approximately 3000 B. C., by a very different type of monochrome pottery after the mound had been abandoned for a considerable length of time.

I may be relieved here from the duty of showing that the sculptures of Tell Halaf by no means certainly belong to the same archaeological layer as the painted pottery. I have discussed the matter at length elsewhere.[5]

The alleged unity of facts, then, vanishes with thorough examination. There remain dissociated features of the culture which may belong in quite different contexts.

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[2] *Iraq* 3 (1936). 3 f. [3] M. E. L. Mallowan, *Iraq* 2 (1935). 1-178.

[4] M. E. L. Mallowan, *Iraq* 3 (1936). 1-59. Cf. also L. W. Woolley on Carchemish in *Iraq* 1 (1934). 158-62.

[5] *Hethiter, Churriter und Assyrer* (1936), 87 ff.

Ungnad locates Subartu in the Mesopotamian plains, and this location is essential for his argument. Nobody has denied this for the IInd millennium. Ungnad's main effort is to establish it for the IIIrd millennium.[6] I cannot find that the evidence he offers is convincing. In fact, it consists of only one passage: the inscription of Narām-Sīn published by Sidney Smith in *Ur Excavations*, Texts I, *Royal Inscriptions* no. 274 (Ungnad, pp. 43 f. and 117 f.). It contains the statement that the king conquered the territory[7] of Subartu as far as the Cedar Forest, i. e. the Amanus (*ibid.* 275, 26). The *iššakkū* of Subartu are mentioned in the sequel beside the lords of the "Highlands."[8] It is true, the passage indicates a larger area of Subartu than had been previously assumed,[9] but the area indicated must be viewed in the light of the contemporaneous geography. The inscriptions of the Akkad dynasty make it perfectly clear that the Euphrates valley and Syria did not belong to Subartu, but were in the hands of the Amurrites. The "Highlands" of the above passage, according to Sargon,[10] include Mari, Yarmuti, and Ibla, and border on the Cedar Forest too. Subartu, then, must be pushed back to the north. It seems certain to me that there did not exist a Subartu which extended from Samarra and Kerkuk to Malatia and Carchemish when the Akkad dynasty conquered Syria and invaded Eastern Anatolia. The threat to the right flank of the conqueror would have been unbearable. Subartu, therefore, is to be located in the mountainous zone. It possibly included the basis of the Upper Tigris and circled around Assyria

[6] It is strange that the author fails to cite the previous discussions of the problem: B. Landsberger, ZA NF 1 (1924), 228 ff.; C. J. Gadd, RA 23 (1926), 65 ff.; Sidney Smith in *Ur Excavations*, Texts I (1928), p. 73; B. Landsberger, OLZ, 1931, 130.

[7] The usage of kalam in this context is exceptional; I refrain, however, from questioning its legitimacy.

[8] Some remarks on this inscription may be pertinent: for the ideogram contained in I 5 compare possibly Deimel 444, 69. Instead of *kà-lí-sa-ma*, the duplicate in Istanbul Asariatika Müzeleri Neşriyati 12 (1934) pl. 5 no. 10 offers *e-ni-ir-ma* (graphically very similar); a verbal form seems indispensable. In II 1 read *Tal-ḫa-tim*^{KI} and compare *Ta/ilḫat* (J. Lewy, *Die Kültepe-Texte der Sammlung Hahn*, 47).

[9] Landsberger has admitted this in OLZ, 1931, 130.

[10] UMBS V 34 + XV 41 col. VI. Cf. B. Landsberger, ZA NF 1 (1924), 232 ff.; Sidney Smith, *Ur Excavations*, Texts I (1928), p. 79 ff.; B. Maisler, *Untersuchungen zur alten Geschichte und Ethnographie Syriens und Palästinas* (1930), 6 ff.

in the north, just as did the Hurri countries of the Hittites and the Nairi countries of the Assyrians. With our present information, we cannot decide whether the region of Tell Halaf was Subarean or Amurritic.[11]

It is a fact which has always been recognized that there were Hurrites (Subareans) in the country east of the Tigris during the period of the 3rd dynasty of Ur. It cannot be seen how their presence, as Ungnad asserts, deals a final blow to his opponents who deny the existence of a Subartu within the Mesopotamian plains at this time. The emphasis laid upon this point might be justified, could Ungnad present a fair collection of Subarean names from Mari, Hana, Tell Halaf, or similar places. The evidence, as it stands, simply shows that after the downfall of the Akkad dynasty —probably in connection with the Gutean movement—Subareans entered the territory between 'Aḍēm and Upper Zab. It is safe to assume that they descended from the eastern mountains and the countries beyond, i. e. from the region where the Hammurabi period locates Subartu.

To sum up, I can find in the sources no support whatsoever for the assumption that Upper Mesopotamia south of the Ṭūr 'Abdīn was ever included in Subartu before the IInd millennium.

The question of the terminology, i. e. whether Subartu is to be preferred to Hurri land, Subareans to Hurrites, seems to me of minor importance.[12] The latter, as the *ḫurwu-ḫe umini* "Hurritic country" of the Tušratta letter shows, has the advantage of being a native term. The existence of a *ḫubur* (pronounce *ḫuwur*?) in the Sumerian column of geographical lists collecting ideograms of Subartu (Ungnad, p. 26) suggests that the term was not entirely unknown to Akkadians. The possibility must be kept in mind that *subar, suwar,* may be a mere dialectical variant of the same term; we know, however, too little of Hurritic and its dialects to make a special point of this.

Though Ungnad's main thesis, in my opinion, is to be rejected,

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[11] Ungnad strangely underrates the significance of the Amurrites. It is in definite contradiction to the sources if he admits (p. 149, note 2) only sporadic and ephemeral Amurritic domination in Upper Mesopotamia.

[12] I believe with Ungnad in the virtual synonymity of the two terms. The designation "man from SU," in tablets of the 3rd dynasty of Ur (Ungnad, pp. 106, 137) must, however, be used more cautiously; only a few names of such people are Subarean.

the book has the merit of presenting the full evidence in the case. New material, it is to be hoped, will in the near future help to clarify further the situation. The publication of the Mari correspondence [13] also seems very promising in this respect. Excavations in a more northern site, best in Fecheriyah (Ungnad, p. 122) are most desirable. Baron von Oppenheim holds the concession; may he be enabled to use it!

A Hittite Glossary. Words of Known or Conjectured Meaning with Sumerian and Akkadian Words Occurring in Hittite Texts. By EDGAR H. STURTEVANT. Second Edition. Philadelphia: LINGUISTIC SOCIETY OF AMERICA, University of Pennsylvania, 1936. 192 pages.

It is the great merit of E. H. Sturtevant to have provided all those interested in Hittite studies, particularly Indo-Europeanists, with an adequate and up-to-date introduction into Hittitology and its different aspects. His previous work to which the *Comparative Grammar* (1933) and the *Chrestomathy* (1935, in coöperation with G. Bechtel) bear witness, has now been rounded out by a new edition of the *Hittite Glossary*, which replaces the first edition of 1931.

Like its predecessor it is a full index to all contributions to Hittite lexicography. In the present stage of Hittitology its compilation was a necessary and useful undertaking. The pertinent literature has become so large and so widely scattered over numerous periodicals and publications that even those who work continuously in this field cannot longer do without such a tool. This time also etymological discussions have been listed, a procedure which will be welcomed by all linguists who wish to benefit by the progress in Hittitology. An etymological dictionary of the Hittite language cannot yet reasonably be expected. Some references to original texts which have not yet been treated as a whole are occasionally included.

The progress made in the five years which have elapsed since the first edition is evidenced by the fact that the book in its present

[13] There is reason for the hope that they may provide us with a detailed picture of the political situation immediately before Hammurabi's conquests in the north. In the pertinent year-dates Mari, Ešnunna, and Subartu play an important part. Just these countries reappear in the specimen published by Thureau-Dangin in RA 33 (1936). 171 ff.

form contains more than twice the number of pages of the previous edition. This increase is only partly due to new items—as a matter of fact not a few items have been canceled as erroneous—but the author has paid much more attention to grammatical forms and morphological derivatives. It is now much easier for beginners to identify forms which depart from the usual pattern.

In a book like this it is quite easy for two different and incompatible readings or interpretations of identical passages to be recorded. This danger, as far as I can see, has successfully been avoided. I have found only two items which are included through such an error: *RIMMU* (p. 127); the passage really reads É *ka-ri-im-mi* (Sommer, who is credited with having established the existence of the word, has corrected himself in *Pāpanikri* p. 13). *IZZI*, gen. of *IṢU* is suspicious because of its ungrammatical form (you expect *IZI*, i. e. *IṢI*); it seems to be parallel to the preceding entry.

The glossary employs a broad transcription of the original syllabic script. It is a necessary evil; any other procedure would cause confusion due to numerous variants. The reviewer has in conversation with the author objected to the use of the macron,[1] particularly in cases like *a-ar-ri*, *e-eš-zi*; according to Akkadian usage the first vowel can hardly mean anything but an initial alif (glottal stop). In *a-a-ra* and *a-a-an-t°* an Akkadologist will believe that a *i̯* was implied (cf. Delitzsch, *Assyrische Grammatik*, sec. ed., § 20 ff.). Another objection was against spellings like *hwnhwes(s)ar*, *hwrzke/a-*; they still do not seem probable to me. Finally I do not believe that a *q* existed in the Hittite phonological system.[2] To my remarks in *Annalen des Muršiliš*, p. 267 ff., should be added the observation that the older Akkadian syllabary, from which the Hittite syllabary is derived, cannot have possessed special signs for the Semitic emphatics. Wherever *qa* etc. do occur, they must, therefore, be read otherwise.

A few detailed remarks may be allowed. The reviewer wishes to make it clear that on all these errors and small corrections he blames himself explicitly; he overlooked them when the manuscript was before him.

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[1] On the use of the macron Sturtevant's introductory remark on p. 15 must be noticed.

[2] This is also Sturtevant's opinion; nevertheless he uses *q* in quoting forms which are included between parentheses after the lemmata.

esnas, gen. of *eshar*: reference to Ehelolf, OLZ, 1927, 29 seems appropriate.

harssan- is the only possible reading, not only because of the "anaptyctic" *a* in forms like *ḫar-aš-ša-na-za* 2BoTU 23A II 51, but also because of *ḫa-ar-aš-ni* KBo II 19 rev. 13 and *ḫa-ar-ša-nu-uš* KUB V 7 rev. 20.

GIŠ*ištananas*: add Götze, *Kulturg.* 151, 158.

istanzassas should be listed separately; it is the name of the deity belonging to the *istanza(n)*-.

kalulupas: with gen. GÌR-*as* "toe" (KBo I 51 rev. 10).

karimn- has become suspicious, since Ehelolf has established *siunas* as the normal word for "god"; it possibly means "temple" the preceding É "house" being a determinative.

GÍN: I am very much inclined to return to this reading; it is definitely favored by the evidence of the contemporary Nuzi texts.

kut(t)as: the nom. is *kuts* (*ku-uz-za*) according to Bo 2002 I 8, 14 (unpublished).

NÍŠ DINGIR-*LIM*: Akkad. *nēšum* (sic!) in this phrase means "live, recover," cf. Thureau-Dangin, RA 18. 154; 23. 26. In Arabic *na'aša* corresponds which explains the Akkad. *ē*.

ŠĀTU is plural; the sing. is *ŠŪTU*; cf. Thureau-Dangin, RA 29. 191 f.

UŠTAHIZZU is to be omitted; it is taken from an Akkadian text.

ZU: cf. under GÍN.

The printing is excellent; with Akkadian words a few wrong or missing accents have remained uncorrected. ŠIBIR is Sumerian, *ŠĪBU* Akkadian and should be printed accordingly.

This excellent tool, for which we are deeply indebted to Sturtevant, will be used with gratitude by all workers in Hittite fields for many years to come. It is to be hoped that some day it may be replaced by a dictionary which will record the complete vocabulary of the published texts with the relevant quotations. But this is a project which is far in the future.

ALBRECHT GOETZE.

Yale University.

Studies in Dravidian Philology. By K. RAMAKRISHNAIAH. Madras: UNIVERSITY OF MADRAS, 1935. Pp. x + 185. 3 sh.

The chief widely differing Dravidian tongues are Kanara, Tamil, Telugu, Tulu, in the south; Brāhui, Gōndi, Kui, Kurukh, in the north. Their interrelations are fairly well shown by their forms of demonstrative pronouns. Kanara has m. *ava* < **awan* < **aan* < **asan*; f. *avaḷu* < **awaḷ* < **aaḷ* < **asaḷ*; m. f. pl. *avaru* < **awar* < **aar* < **ahar*; n. *adu* < **atu* < **at* < **aδi*; n. pl. *avu* < **awi*. Tamil has m. *avan*; f. *avaḷ*; m. f. pl. *avar*; n. *atu*; n. pl. *avai* < **awas*, an accusative used for the nominative. Tulu has m. *āje* representing **ā* < **aan* < **asan* combined with the ordinary nominative-ending *-e*; f. *āḷu* < **asaḷ*; m. f. pl. *āru* < **ahar*; n. *au* < **aδu* < **aδ* < **aδi*; n. pl. *aikuḷu* for **ai* < **awi*. Telugu has m. *vān-* < **asan*, nom. *vāḍu* < **awāṇḍu* < **aaṇṭru* < **asarṇṭu* < **asanuṇṭu*; m. f. pl. *vāru*; n. *adi*, used also for the feminine; n. pl. *avi*. It has lost the f. **aaḷ-* < **asaḷ*, nom. **aaṇṭru* < **asaḷuṇṭu*, because of formal confusion with the masculine. Gōndi has m. *ōn-* < **aun* < **awan* < **aan* < **asan*, nom. *ōr* < **awaṭru* < **aaṇṭru* < **asanuṇṭu*; m. pl. *ōṛ* < **ahar*; n. *ad* < **aδi*; n. pl. *au* < **awi*. Kui has m. *aan-* < **asan*, nom. *aandžu* < **aaṇḍru* < **aaṇṭru* < **asanuṇṭu*; m. pl. *aaru* < **ahar*; n. *āri* < **aδi*; n. pl. *āvi* < **awi*. Kurukh has m. *ās* < **asan*; m. f. pl. *ār*; n. *ād*, used also for the feminine; n. pl. *abṛā*, perhaps from Aryan *sarva* (all). Brāhui demonstratives lack gender-forms.

From this outline it is clear that the eight tongues differ greatly in fonology and morfology, except that Kanara is rather closely connected with Tamil. Any serious historic study of Dravidian must take into account at least these eight tongues. Ramakrishnaiah's book does not give a single word-form from any one of the northern tongues. It therefore cannot be considered a really scientific work.

The first chapter deals with the general history of Dravidian and the Dravidians. The second and third chapters are made up of speculations on the source of various verb-suffixes and noun-suffixes. The fourth chapter discusses nouns and their relations to verbs.

In these four chapters I find little of importance. Some of the author's statements are surprising. In § 3 we read that the excavations at Mohenjo-daro and Harappa "go to prove . . . the identity of their civilisation with that of the Dravidians." This is news to me. § 13 seems decidedly unlucky. Here the author begins his

discussion of Dravidian speech with a statement that *var* (come), *tar* (bring), *ī* (give) and *veḷ* (shine) are primary roots. A knowledge of Kui would have shown him that the *r* of *var-* and *tar-* is not radical; it represents the personal-object suffix. Moreover *ta-*, with the variants *tai-* and *te-*, is a compound made up of **et* and a demonstrative *ai*-suffix; and *ī-* was extracted from a compound of **et* and an *i*-suffix (*AJP* 50. 153). Further on we read of the pronominal roots *nā* (I) and *nī* (thou). These are not root-forms; they are alterations of basic *ēn* and **īs*, lengthened from *en* and **is*, as I have shown in *Dravidian Developments*. On the same page we read: "Unlike the roots in Sanskrit or other Indo-European Languages . . . never found in actual use . . . Dravidian roots are used . . . as a form of imperative: *pō* = go, *tar* = bring, *paḍu* = fall." Universal negatives are dangerous. Allowing for fonologic changes, Latin *ī* (go) and *fer* (bring) and English *go* and *bring* are root-forms just as truly as *pō* and *tā*. I do not know where the author found such a word-form as *tar*; there is a present-stem *tar-*, replaced by *ta-* in the past and by *tā* in the imperative, in Kanara-Kēlan-Tamil. And *pō* is hardly a root-form; the root was rather **bo* or **bå*: the short vowels *o* and *a* are found in the imperative of Telugu, which seems to be the author's native tongue; *b* is found in Kurku *bă* (go). As Kolarian does not seem to show voicing-changes of initial occlusives, we may assume that *bå* represents Dravidian **bo* or **bå*. Telugu *paḍu* is not a root, nor even a stem; the stem is *paḍ-*, and the root perhaps **paṭ*.

In § 14 we read: "Strictly speaking, there is nothing like a case in these languages." Kanara has *ānu* < **jān* < **ēn* (I) with the genitive *ena*; Tamil has *jān* and *-ēn*, g. *ena*; Telugu has *ēnu*, g. *nā* < **enā*; Kui has *ānu* < **ēn*, g. *nā* < **enā*; Gōndi has *anā* < **ēnēn*, g. *nā-* < **enā*; Brāhui has *ī* < **ēn*, g. *kanā* < **enā* (*Dravidian Developments* § 65); Kurukh and Malto have *ēn*, with evidence of a former genitive **enā* or **ena*. We know, as well as anything historic can be known, that early Dravidian *ēn* had the genitive *ena* or **enā*. This genitive was just as truly a case as *tuī* is a case of *tū* in Latin, or *its* a case of *it* in English. It is possible that *ena* **kutiras* (my horse) developt from *en a* **kutiras* (me that horse), but that does not alter the fact that *ena* is and long has been a genitive.

In chapter 2 the author discusses Caldwell's theorizing about the past-suffix *d* or *t*, and concludes that it represents **itu* from *ī-*

(give). Kanara *ī-* has the past-stem *itt-*, which contains the *t*-suffix, so the explanation explains nothing. We simply know that early Dravidian had *t* or *d* as a suffix of the past and *tt* as a suffix of the perfect (*AJP* 50. 150): that is the limit of our knowledge on that point.

In chapter 3 the author assumes that **avan* was the early form of Telugu *vān-* (that), and that it developt by a mysterious fonologic process thru **avanu*, **avandu*, **vāṇḍu*, to the nominative *vāḍu*; and he declares that the *-ḍu* cannot properly be called a nominative-ending. A knowledge of Kurukh would have taught him that Tamil *avan* and Telugu *vān-* are derived from **asan*. In nearly all varities of early Dravidian there was a tendency to drop final consonants. The m. **asan* and the f. **asaḷ* became confused in a single form **asa*, leading to the use of the neuter singular for the feminine in Kurukh-Malto. In Gōndi-Kui and Telugu an attempt was made to remedy the ambiguity of **asa* by adding *uṇṭu* (being) to the stems: these tongues developt the nominatives **asanuṇṭu* and **asaḷuṇṭu*. But fonetic changes leveled these forms too: **asanuṇṭu* became **asarṇṭu* and **aaṇṭru*; **asaḷuṇṭu* changed thru **asaṇṭḷu* to **aaṇṭru*, and the neuter singular had to be used for the feminine. The leveled form became m. **awaṭru*, **awaRu*, **auru*, *ōr* in Gōndi; *aandžu* in Kui; **awāṇḍu*, **vāṇḍu*, *vāḍu* in Telugu. The *-ḍu* of *vāḍu* is therefore a true nominative-ending. The author is likewise wrong in declaring that *-lu* is not a nominative-ending in the plural of Telugu nouns. In Telugu *strīlu*, gen. *strīla*, dat. *strīlaku*, acc. *strīlanu*, voc. *strīlārā* (women), *-lu* is a nominative-ending just as much as the *-ēs* of *mulierēs*, or rather more so, for *mulierēs* is accusative and vocative also, while *strīlu* is nominative alone. Of course the *-lu* is a plural-ending too, as the author says; but so is Latin *-ēs*.

In chapter 4 the author assumes *bi*, with the variant *vi*, as a noun-suffix. The only example of the *bi*-suffix given is Kanara *kibi* [!] (ear). The Kanara word is *kivi*; *kibi* is hardly possible as a native word-form in Kanara. This *kibi* is put beside Tamil *kēlvi* (hearing) and *kēl-* (hear). The Tamil word is *kēḷvi* from *kēḷ-*; it cannot be compared directly with *kivi*. Tamil has two words for "ear," *cevi* = Kanara *kivi*; and *kātu*, with *ā* < *au* < *ou* < *eu*, corresponding to Kurukh *xebdā*. Perhaps these words are connected with *kēḷ-* < **keḷ*; we might assume *kivi* < **kḷwis* and *xebdā* < **kḷwiδa*. Tulu *kebi* seems to call for **kedwis* < **kḷwiδis*, which would do equally

well for Kui, as Kui has *r* for intervocalic *d*: *kriu* < **kediu* < **kedwis* < **klwiδis*.

At the end of the book is an appendix of 46 pages, in which the author attempts to give the root-forms of a large number of words, chiefly verbs. But he does not show any true understanding of the basic principles of fonology. His first example is Kanara *aRi-*, Tamil *aRi-*, Telugu *eRug-* (know), with an assumed root *aRi*. He ought to know that Dravidian *R* (voiceless *r*) is a derivative of something else, namely *l* or *n* or *r* in contact with a voiceless sound. The fourth example is Kanara *uri-*, Tamil *eri-*, Telugu *eri-* (burn), with a highly unreasonable root *eri*. A possible southern basis might perhaps be **ori*, but I think it is better to assume **wri*, which by combination with Brāhui *huš-* < **urs* < **uris* becomes **wris*. The crowning example of weak fonology seems to be *sā*, given as the root of Kanara *sā-*, Tamil *cā-*, Telugu *ca-* (die). Kanara *s* regularly represents an older *c*. Tamil has the past tense *cettēn*, with *e* evidently older than the *ā* of the present tense; cp. *ena* (my) beside *jān* < **ēn* < **en* (I). The northern *k*-forms show that *cā-* and *ce-* came from **ke-*, or perhaps from **ge-*. So *sā-*, instead of being the "probable primitive form," as the author calls it, is the form furthest away from the root.

EDWIN H. TUTTLE.

Washington, D. C.

Elements of Buddhist Iconography. By ANANDA K. COOMARASWAMY. Cambridge: HARVARD UNIVERSITY PRESS, 1935. Pp. viii + 95, with 44 plates and explanation. $3.50.

La Sculpture de Bodhgayā. Par ANANDA K. COOMARASWAMY. Ars Asiatica XVIII. Paris: LES ÉDITIONS D'ART ET D'HISTOIRE, 1935. Pp. iv + 74, with 1 plan and 70 plates. 200 francs.

Dr. Coomaraswamy's *Elements of Buddhist Iconography*, with his *Yakṣas* and numerous other publications, explains a number of motifs in Indian art and helps to transfer them from the category of ornamental to that of significant. Every one of these writings is a contribution of high importance and the result of prolonged research and acute analysis. In studying Buddhist iconography he justifiably seeks origins in pre-Buddhist sources and looks for enlightenment from contemporary non-Buddhist material. It is a

method which produced good results in his study of the origin of the Buddha image, and is indicated for the examination of all Buddhist types.

In his *Elements of Buddhist Iconography* he is trying to establish Vedic sources for the Buddha's Wisdom Tree, which he equates with the Vedic Tree of Life or World Tree; the Buddha's Lotus Seat, which he identifies with the World Lotus; the Buddhist Wheel of the Law, which he associates with the Vedic World Wheel; and the place of the Buddha's Lotus Throne, which he finds is the central point of the universe, the seat of Agni. The amount of material which he has collected on all these points, almost all of it new, is impressive; and he has established the point that there is a connection between the Buddhist symbolism and Vedic materials.

But there is an implication, which sometimes seems so strongly expressed as to exclude any other interpretation, that Buddhist symbology from the beginning has contained all the mystical content that can be found in the most advanced Vedic texts and later Buddhist literature. This point of view I find it difficult to accept. Take, for example, the Buddha's Wisdom Tree and the Vedic Tree of Life. I do not believe it is correct to state, or imply, that the Buddhist tree of enlightenment has always, and among all Buddhists, had the *full* significance that can be attached to the Vedic Tree of Life in the most esoteric (*brahmodya*) type of Vedic thought. That type of Vedic thought is consciously recondite—as in RV. I. 164, which does not mean to be intelligible to any but the initiated. In finding origins and explanations for the Buddha's tree, I should attach considerably more importance than I find expressed in Dr. Coomaraswamy's book to the folklore notions of tree-worship, which appears to go back to the Indus Valley seals. Indeed that sort of approach to the problem is followed in Dr. Coomaraswamy's Bodhgayā, as it was in connection with other problems in his *Yakṣas*. In a historical survey of the symbol and its use, the folk beliefs should probably come first; later the type so established was rationalized and the interpretation elaborated by mystic speculation. The situation with Indian plastic symbolism seems to me comparable with that of word treatment in the Nirukta and the Mīmāṅsā, which do not give correct etymology, that is, origin of the words, but rather the mystical association of words by sound for esoteric exegesis and application.

The appeal of early Buddhism seems to have been to the folk, at least down to the time of Asoka and the first Buddhist monuments, and it is to be presumed that to the folk whom Asoka's pious inscriptions and noble columns were to edify the meaning of the symbols was direct and obvious, not remotely speculative and mystical. Possibly even at Asoka's time advanced mystics were giving remote interpretations to the symbols, but Asoka's own simple words on his columns would not indicate that he was moved by their speculations and thought of them as the ideas he wanted to symbolize. The wheel at Sarnath meant the Law and its preaching, but it did not also mean everything else connoted by the wheel to late Vedic thought and esoteric Buddhism. Later these recondite ideas may have come to influence the plastic art, especially in the ornament of monuments belonging to mystical sects. Dr. Coomaraswamy's splendid study, therefore, seems to me open to some modification in the line of historical treatment, so as to work out the symbology from origins, probably simple and obvious, to more specialized application of varying sorts. That is to indicate that there were more varieties of Buddhism than an advanced mysticism, just as there were more varieties of mediaeval European Christianity than the kind represented by Eckhart, and that these other varieties hold as important a place, perhaps even a more important place, in giving motifs to the plastic art.

His study of Bodhgayā is the sort of thing which we need literally in dozens for India, yet have only in a handful. It is a complete survey of the ancient site—its history, its architecture, its sculpture, its iconography. It even summarizes inscriptional material, although Dr. Coomaraswamy states that this topic is beyond his full control and the book's scope. It covers, on the topics treated, the literature to date of publication, advances well substantiated new explanations of some details, and points out what is still to be explained at the site. There are excellent illustrations of all the remains. It is a scholar's guide and source book.

Indian Pictorial Art as developed in Book-Illustrations. By HIRANANDA SASTRI. Gaekwad's Archaeological Series, No. 1. Baroda: BARODA STATE PRESS, 1936. Pp. iv + 24, with 19 plates. 13 as.

The Gaekwad of Baroda is justly honored as one of the most able rulers of modern India, whose state shows the result of his interest in education, public health, and his people's welfare in general, and whose outside activities show his concern with human questions on a large scale. A man of great appreciation for things of the spirit as well as those of the flesh, he has established a library and research publication department at Baroda, which has now attained an unexcelled position in Indic scholarship for its Gaekwad's Oriental Series. He has recently established a department of achaeology for his state, and appointed as its director Dr. Hirananda Sastri, retired Government Epigraphist for India. In view of the many important archaeological monuments in the Baroda state and the possibility that sites existing there will on excavation prove important, this new venture is one of significance to Indic scholarship.

In the first volume of the Gaekwad's Archaeological Series the director sets out to refute the alleged opinion of other scholars that India did not have a pictorial art of book-illustration. In recent years, certainly, no one has held this opinion, and Dr. Sastri's thesis seems a man of straw hardly worth while for anyone to attack.

In his discussion he adduces some interesting diagrams from manuscripts, which come under the head of book-illustration, although hardly of pictorial art, and are valuable in showing the early history of a phase of book-illustration, at least allied to pictorial art. He also cites some literary passages on mudrās (hand-poses) of the sort illustrated in some manuscripts.

The main part of his booklet deals with painting from Gujarat. Most of his material is of small importance, as adding little to known and published types of illustration, but he shows two illustrated folios of a manuscript of Hemacandra's Siddhahemalaghuvṛtti, not dated, but thought by Dr. Sastri to be of the 12th century. It may be; but the paintings, as far as I can see from the very small and poor reproductions, are much like dated illustrations I know in 13th century manuscripts.

He also, strangely, advances an illustrated paper manuscript of the Jaina Kalpasūtra as a work of Vikrama Saṃvat 1125 (A. D. 1068).

He bases his opinion on the colophon, which reads *vikramasaṃvat 1125 dīpamālādine saiddhāntikaśiromaṇinemicandrasūriṇā svacitkośe sthāpitam*. Since no such early paper manuscript is known for India, this would be a startling discovery; so too its illustrations would be the earliest known miniature paintings from western India. The date and Dr. Sastri's conclusions are fallacious. It is to be noted that the colophon does not claim to make any statements about the time when the manuscript was copied; it merely states that "On Diwali day, in the year Vikrama Saṃvat 1125, the sūri Nemicandra, crest-jewel of the Scripture, fixed [the text] in his heart (i. e., committed it to memory?)." The handwriting of the colophon is obviously different from that of the rest of the work, as appears in Dr. Sastri's reproductions; notice the difference in making *d* (especially the "tail"), *v* (squarer in the colophon than elsewhere), *m* (much larger opening in the colophon), *s*, *p*, *k*. The final sentence is a later addition; how the erroneous date got into it I do not venture, or need, to guess. The very appearance of the manuscript is against this manuscript being early, even for paper, which became common for books in western India around 1400. In the earliest known reliably dated paper manuscripts the proportion of dimensions is closer to that of the dimensions of the palm folios. Here the proportions are those of positively dated manuscripts of the early 16th century. The folios of this manuscript compare almost exactly with those of manuscript Hc in my *Miniature Paintings of the Jaina Kalpasūtra* (see p. 3, and Fig. 15), which is dated Vikrama Samvat 1577 (A. D. 1520). Similarly, Dr. Sastri's manuscript has the same number of lines to the page, the same large writing, the same style of festooned lozenge-ornaments to mark the (old) string-hole spot and page-number spots. The paintings are placed in the same way, and the whole appearance of the page is similar. Many other 16th century manuscripts of the same style are known; and the 16th century, or possibly late 15th century, is the earliest date that can by any means whatever be assigned to his manuscript. The style of painting is also similar. Earlier dated paper manuscripts appear in my *Kalpasūtra*: see B 17. 2277, Hd, Hf, M 18. 104. For manuscripts, all palm, of a period only shortly after that to which he assigns his paper manuscript, see in my *Kālaka*, plates 1, 2, and his own plate 10.

W. Norman Brown.

University of Pennsylvania.

La Peinture Iranienne, sous les derniers Abbasides et les Il-Khāns. Par IVAN STCHOUKINE. Bruges: IMPRIMERIE STE. CATHERINE, 1936. 188 pp., XLVI plates.

The History of Persian Painting, as Dr. Stchoukine properly lays down in his preface, now finds itself facing the delicate study of problems of detail. To this study he devotes his witty, lucid, and judicious book. After an admirable résumé of the theories of his predecessors in the field, upon whose militant asseverations his courtesy is not the least pointed of his comments, he assembles a much-needed corpus of manuscripts and of historical references, and embarks on a detailed analysis of the paintings in his list.

This analysis seems to establish his general thesis. Abbasid painting, in spite of heterogeneous influences, is predominantly Iranian: far-Eastern influences which are in full play at the beginning of the fourteenth century gradually give way to the calmer line and gay polychromy of the old Iranian style. So far (and so far, it is to be observed, he follows closely the scheme indicated by M. de Lorey),[1] and in his discrimination of three successive Mesopotamian styles, primitive, realistic, and stylised, his analysis supports the validity of the categories he employs.

But minuter attention suggests that an important factor in the formulation of the mature Persian style is neglected by Dr. Stchoukine. Although he mentions Ibn Bawwab the calligrapher as a fresco-artist, and takes al-Wasiṭi, the calligrapher-painter of the Schefer Hariri, as the typical master of his second Mesopotamian period, he barely pays the customary lip-service to calligraphy as a decisive factor in the evolution of Persian painting. From this omission follows his mistaken attribution of the characteristic fine line to far-Eastern sources. The turbans and drapery of the Hariri Schefer,[2] and, still more definitely, such drawings as the British Museum sketch (repr. Glück & Diez, *Kunst des Islam* 503b) display a line as fine as any in the Mongol period. Again, the asymmetrical compositions of that period derived from older Iranian formulae (cf. the "Death of Moses," in the Jāmi' al-Tawarikh,[3]

[1] "La Peinture Musulmane. L'Ecole de Bagdad" (*GBA* 1933) and "L'Ecole de Tabriz" (*RAA* 1935).

[2] A list of reproductions is given by Stchoukine, p. 71, No 3.

[3] Blochet, *Musulman Painting*, Pl. XLIX.

with "Erasistratus and his Famulus" in the Sarre collection, Berlin).[4]

Dr. Stchoukine neglects ceramic painting, an invaluable index of the pictorial art destroyed, in its more destructible vehicles, by the Mongol invasion. The figured pottery, of Rayy, Kashān, and Sāva, represents a school of painting, flourishing in central Persia in the twelfth and thirteenth centuries, from which many of the most critical characteristics of Mongol and Timurid art derive. Here is a lively naturalism expressed in purely calligraphic line. Here, long before the Mongol invasion, is a round, linear-featured face of "Mongol" type which vitiates Dr. Stchoukine's analysis (p. 138), and offers a sharp contrast with the heavy-featured face of Raqqa ceramics, so close to the late Mesopotamian illustrations. That this linear style was not confined to ceramics the existence of some frescoes from the Kevorkian collection[5] offers proof.

A just formulation of the component elements in Mongol painting is impossible without the collation of more material than Dr. Stchoukine offers. But even if his categories were too hastily chosen, and his order imposed rather than educed, nevertheless his book strikes an attitude of careful observation which future writers will probably be wise to imitate. His studies pick out Byzantine, Chinese, or Sasanian features, all perhaps really external to the body of an art which was organic and, from Saljuq times at least, continuous; but the native features must be traced with the same conscientious care as Dr. Stchoukine's, by those who wish to establish its claim to originality on a scientific basis.

ERIC SCHROEDER.

Cambridge, Mass.

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[4] Kühnel, *Islamische Miniaturmalerei*, Pl. 5.

[5] One of these is in the possession of Fogg Art Museum, Harvard College. Others are now (Jan. 1937) on exhibition there.

Saṃdhinirmocanasūtra, l'explication des mystères. Texte tibétain édité et traduit par ÉTIENNE LAMOTTE. Recueil de travaux publiés par les membres des CONFÉRENCES D'HISTOIRE ET DE PHILOLOGIE, 2e Série, 34e Fascicule, Louvain: 1935. Pp. 278. Price 30 belgas; 75 French francs.

The Saṃdhinirmocanasūtra [1] is another important Buddhist text whose Sanskrit original has been lost. It has been preserved only in translation: one Tibetan and five Chinese,[2] of which two are incomplete. In addition there are four commentaries in Tibetan, one of which is a translation of Yüan-ts'ê's 圓測 Chinese commentary. Dr. Lamotte has edited [3] the Tibetan text and translated it with line and page references to Hsüan-tsang's translation. Of the commentaries only the one by Asaṅga has been used.

This text is a catechism of Mahāyāna Buddhist doctrine, valid, according to the author, for the first centuries of the Christian era. It would seem to have been compiled gradually of different short texts during the second century and to have reached final form by the beginning of the third. It is slightly later than the Daśabhūmika and the Kāśyapaparivarta, and contemporary with the oldest parts of the Laṅkāvatāra. Dr. Lamotte has made a genuine contribution to our studies by making easily available a text that should be very useful as an introduction to Mahāyāna Buddhism. Both the catechistic style of the text itself and the numerous commentaries and translations provide excellent material for a first seminar in Buddhism.

The present volume is an excellent piece of work, and the reviewer is glad to recommend it; he hopes that it will be consulted by all interested in Buddhism, and, particularly, that it will receive the extensive use in classes that it deserves. It exhibits the high degree of care and thought to be expected of a man who has had the good fortune to work for ten years under the tutelage of such a master as Professor Louis de La Vallée Poussin.

The edition of this text should provoke among Sanskritists a

[1] This form is guaranteed by the Tibetan transliteration. The Tibetan title is *Ḥphags-pa Dgoṅs-pa Ñes-par ḥgrel-pa Theg-pa Chen-poḥi Mdo.*

[2] Nanjio 154-156, 246, 247. Lamotte says nothing about the lost Chinese commentaries; cf. *Bukkyōdaijii* 1028C.

[3] The transcription of Das's dictionary is followed with the one exception that *zh* is used for *sh*.

series of articles in the attempt to clarify the semasiology of *saṃdhi* and the verb *saṃdhā*. Wassiljew [4] has translated the title of our sūtra as "Erläuterung des Willens, wörtlich Lösung der Verbindung." Bendall and de La Vallée Poussin [5] would translate as "mystery" or "secret." Lamotte recalls that the Tibetan rendering of the word, *dgoṅs-pa*, "purpose" or "intend," is most frequently used to translate *abhiprāya*, "intention." He then assumes that *dgoṅs-pa* and *saṃdhi* are possibly abbreviations for *ldem-por dgoṅs-pa* and *abhisaṃdhi*, "intentional teaching," and suggests the translation "hidden intention." Paramārtha's Chinese translation renders this difficult word literally by *chieh* 節, "joint." The two partial translations attributed to Guṇabhadra employ the term *hsiang-hsü* 相續 which is the equivalent of either *saṃdhi* or *saṃtāna*. On the other hand, both Bodhiruci and Hsüan-tsang translate the word by *shên mi* 深密, "profound secret." [6]

After having admitted on p. 12 that the title of his text means literally "Sūtra détachant les noeuds," Lamotte (cf. especially p. 20-21) misunderstands the title of Paramārtha's translation. It is impossible for me to believe that *chieh* can represent here anything other than a translation of *saṃdhi* whereas Lamotte would take the word with the preceding *chieh* 解 as the translation of *nirmocana*. He would have done better to follow Nanjio.

It is surprising, to say the least, that in the list of sources no mention is made of Yüan-ts'ê's *Chieh-shên-mi-ching Su* 解深密經疏 published in *Kyōto, Suppl.*, 1. 34. 4, 5 and l. 35. 1, while the Tibetan translation of this commentary is noted. On p. 17 Lamotte asks a question which I find incomprehensible: "Pourquoi existe-t-il un commentaire de Ven-tshig sur le chap. IX seulement?" "Ven-tshig," as Lamotte well knows (cf. p. 11), is the Tibetan transliteration of Yüan-ts'ê (sixth century Yiwän-ch'iak), who, according to a brief notice in *Sung Kao-sêng-chuan* [7] 4 (Taishō 50. 727B), was a student in the time of Hsüan-tsang. His commentary

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[4] *Der Buddhismus*, St. Petersburg, 1860, p. 152.

[5] *Muséon*, 1906.

[6] The translation of the title of our sūtra given in Sakaki's *Mahāvyut.* 1359, *Kuang-k'ai Chên-shih Ching* 廣開眞實經 (something like "Revealing-truth-sūtra"), may be passed over as a late, euphemistic, vague rendering.

[7] Cf. also *Liu-hsüeh-sêng Chuan* 22 六學僧傳, *Kyōto, Supplement*, 3. 6. 5. 420aA.

was originally on the ten chapters of the *Saṃdhi°*, but the text published in two and one half volumes of the Kyōto tripiṭaka lacks the last chapter, the commentary to chapter 10 of the *Saṃdhi°*. Since nothing is said to the contrary, it is reasonable to suppose that Yüan-ts'ê's work has been preserved intact in the Tibetan translation, for it covers three volumes in the Tanjur. Therefore, Lamotte's question is to be deleted.

Given the difficulty of access to editions of the Kanjur and Tanjur, it may be helpful to students if the reviewer appends to this notice the variations he noted between the text as edited by Lamotte and the Narthang edition of the Kanjur. For this purpose the following pages of Lamotte's edition have been collated: 31-45, 88-97, 149-166. **32,** *6 f.(rom) b.(ottom)*, thams cad kyis ye; *5 f. b.*, med pa / dam; *3 f. b.*, daṅ dbuṅ med. **33,** *5*, legs par bsam pa. **35,** *10 f. b.*, rnams kyi ḥphags. **36,** *10*, mtheṅ bas brjod du; *20*, bai ḍūrya daṅ; *22*, tshogs rnam par bstan na /; *30*, sñam du sems çiṅ /. **37,** *6*, baṅ baḥi tshegs. **38,** *18*, byas daṅ / ḥdus. **39,** *6 f. b.*, so—sñam; *4 f. b.*, gsol pa daṅ. **41,** *2*, pa ni naṅ; *13*, bdag gi yoṅs. **42,** *9*, daṅ / don; *13*, daṅ / don dam. **43,** *1*, gsol pa daṅ; *11 f. b.*, kyi mtshan ma. **44,** *1*, par rṅogs par ḥtshaṅ; *16 f. b.*, daṅ / don dam. **45**, *18 f. b.*, daṅ / don dam; *8 f. b.*, med pas [tsam. **88,** *15 f. b.*, brñan daṅ; *7 f. b.*, daṅ / dgoṅs pa. **89,** *11*, sems dpas legs. **90,** *4 f. b.*, ma yin [ma yin]. **91,** *4*, çes pa ni dmigs; *10*, yin·pa de de ltar. **92,** *15 f. b.*, pa / rnam. **93,** *5*, po pa daṅ /; *10 f. b.*, rjes su brtan pa; last line, rjes su ḥbaṅ ba dbaṅ. **94,** *9 f. b.*, gzhol pa / byaṅ; *7 f. b.*, / gnes gyur pa; *6 f. b.*, ba la ḥbab pa [43^{b}] /. **96,** *7 f. b.*, yin pa de ni /; *4 f. b.*, snaṅ ba ḥol byi. **97,** *8*, med ciṅ dpyad. **149,** *3*, chos sku zhes. **150,** *11*, dkaḥ ba spyed pa; *12*, tu ston po daṅ. **152**, *5*, la logs par. **155,** 4, rkyen gzhi; *12*, pa daṅ / rkyen. **156,** *1*, mi brtag pa; *4*, pa de ni mṅon; *7*, rtag par rigs; *3 f. b.*, ba daṅ / de. **158,** *7 f. b.*, / de [la gzhan; *6 f. b.*, pa bsten paḥi; *5 f. b.*, ñid kyi yoṅs. **160,** *7 f. b.*, / zaḥo byed do /. **164,** *3*, rigs çiṅ sā; *14*, lus phun sum tshogs. **165,** *9 f. b.*, dpal ḥdi de. **166,** *3*, dgoṅs pa ṅes.

Tibetan Literary Texts and Documents concerning Chinese Turkestan. Selected and translated by F. W. Thomas. Part I: Literary Texts. Oriental Translation Fund, New Series, Vol. 32. London, 1935. Pp. x + 323.

Professor Thomas has collected into this volume translations of, with one exception, all the Tibetan literary references to Chinese Turkestan. The Tibetan materials secured by Sir Aurel Stein will be discussed in Part II of the publication which will form a second volume. While all of us are grateful for this genuine contribution, it is most regrettable that there seems to be no intention of printing the texts themselves. So great, however, are the difficulties attendant upon any attempt to obtain Tibetan books, that no Tibetanist should feel his job complete until he has published the text of any translation. Such is the worthy example set by Dr. E. Lamotte in his *Saṃdhinirmocanasūtra,* and it is earnestly hoped that Professor Thomas will agree to this principle in practice.

The present volume contains careful, annotated translations with introductions of the following material: 1, *Gośṛṅga-vyākaraṇa* or *Ri-glaṅ-ru Luṅ-bstan-pa* (The Prophecy of Gośṛṅga), *Narthang Kanjur, mDo* 30(*a*).336b⁶-354b⁴; 2, *Arhat-Saṃghavardhana-vyākaraṇa* or *dGra-bcom-pa dGe-ḥdun-ḥphel-gyi Luṅ-bstan-pa* (The Prophecy of the Arhat Saṃghavardhana), *Narthang Tanjur, mDo* 94(*ṅe*).412-420; 3a, *Liḥi-yul-gyi Luṅ-bstan-pa* (The Prophecy of the Li Country), *Narthang Tanjur, mDo* 94(*ṅe*).420b³-424a; 3b, *Li-yul-gyi Lo-rgyus* (The Annals of the Li Country), *op. cit.* 424b¹-444a⁴; 4, *Dri-ma-med-paḥi-ḥod-kyis Shus-pa* (The inquiry of Vimalaprabhā), *Narthang Kanjur, mDo* 13(*pa*).339-418; 5a, *Sthaviropanimantraṇa* (Invitation of Elders), *Narthang Tanjur, mDo* 94. 403b⁴-412b⁶; 5b, *The Sandal Image of Buddha, Narthang Tanjur, rGyud* 85(*ru*).142a¹-144a⁵; 5c, Selections from the *Padma-bkaḥi Thaṅ-yig* (Writing of the Pronouncements of Padma[saṃbhava]), which circulates in a separate xylograph volume and is not to be found in either the *Kanjur* or the *Tanjur*; 5d, Selections from the *U-rgyan-guru-Padma-hbyuṅ-gnas-kyi sKyes-rabs-rnam-thar* (Account of the Lives of the Guru Padmasaṃbhava of Udyāna), which also circulates only in separate editions; 5e, Selections from the *rGyal-rabs-gsal-baḥi Me-loṅ* (Mirror Exhibiting the Royal Lineage), which circulates only in a separate edition; 5f, A selection beginning at p. 50a⁶ from the *bKaḥ-ḥgyur-rin-po-cheḥi gSuṅ-par*

Srid-gsum rGyan-gcig rDzu-hphrul-çiṅ-rtaḥi dKar-chag Ño-mtshar-bkod-pa-rgya-mtshoḥi lDe-mig (Key to the Marvellous Ocean of Compilations, An index to the Miraculous Vehicle, the Single Ornament of the Triple World, the Printed Word of the Precious Kanjur); 6, *Li-yul-chos-kyi Lo-rgyus* (The Religious Annals of the Li Country), from a Tun-huang MS now catalogued at the French Bibliothèque Nationale as *Fonds Pelliot tibétain No. 254.*

JAMES R. WARE.

Harvard University.

Yuen Ming Yuen, L'Oeuvre Architecturale des Anciens Jésuites au XVIII^e Siècle, by MAURICE ADAM. Pei-p'ing: IMPRIMERIE DES LAZARISTES, 1936. Pp. xiv, 44. Profusely illustrated.

Yuen Ming Yuen, or Yüan Ming Yüan, is the name of the most famous suburban palace of the Manchu emperors of China. It was one of a large group of imperial villas lying in a well-watered and historic region extending from five to ten miles northwest of old Peking close to the present sites of Yenching and Tsinghua universities. The climax of the palace building in this region, as well as the climax of the splendor and prestige of the Manchu dynasty, came during the reign of Emperor Ch'ien Lung (or K'ien-long), 1735-96, and at this time Jesuit missionaries at his court were employed to construct a set of European palaces in a small park immediately adjacent to the walls of the Yüan Ming Yüan.

Maurice Adam, who died in 1932 at the age of forty-three, had collected considerable material which he intended to use in a history of the imperial palaces of China. That part of his work which dealt with the Jesuit missionaries and the European buildings at the Yüan Ming Yüan was so nearly complete at the time of the author's death that his friends arranged to have that part edited and published as a memorial to him. The title of the work as published should not be understood as indicating a complete treatment of the whole Yüan Ming Yüan.

In his descriptions of the European buildings M. Adam gives us some strikingly new information, *e. g.,* on the festival of the Yellow Flowers in the Labyrinth; the use of two of the buildings

by the famous Hsiang Fei (Ch'ien Lung's Mohammedan concubine); and the painting of the panorama of a foreign town in western perspective on brick walls to represent the city of Aksou (home of Hsiang Fei), instead of a European town as had been supposed previously. This information would be more valuable to students of the history of this palace if the editors had supplied references to Adam's authorities for these interesting statements.

The work consists in part of long quotations from the sources. One of these is the famous letter from Father Attiret in 1743 describing the Yüan Ming Yüan before the construction of the foreign buildings (pp. 3-12), while others describe the making of the fountains and the copper engravings; the chapter on the burning of the Yüan Ming Yüan in 1860 consists almost entirely of quotations from several French writers blaming the English alone for the destruction of the palace, but making no reference to the English writers who usually complain that the looting of the palace was begun by the French before the English arrived.

Many of the best drawings and maps were contributed by M. Kin Hsiun (Chin Hsün 金勳); some of these are new drawings and some copies of original architect's plans for the builders, handed down in M. Kin's family from the time when his ancestors were contractors in charge of the construction of certain parts of the European buildings. M. Adam has made a real contribution in his discovery of this material in Chin Hsün's family archives.

C. B. Malone.

Colorado College.

Les Fêtes Saisonnières au Japon. (Province de Mikawa). Etude descriptive et sociologique. Par N. Matsudaira. Paris: G.-P. Maisonneuve, 1936.

This study is, with the addition of an introduction and a brief conclusion, divided into three parts of unequal length. The first of these (pp. 19-74), is devoted to a description of festivals celebrated yearly by the inhabitants of certain villages in the province of Mikawa. These festivals are: (1) the *Hanamatsouri* or feast of flowers (Matsudaira's transliteration of Japanese words, it may

be noted, is that employed by the French, not that of the *Romajikwai*); (2) the closely related *Mikagoura,* which may well be the ancient form of the *hanamatsouri*; (3) the *Kagoura,* which has not been celebrated since 1855 and which, unlike other similar festivals, came but once in seven years; and (4) the *Dengakou,* a fête that is observed not, like the others in November, but in January. A brief note on summer festivals, held in July, and another on spring and autumn festivals, held in May and mid-October, complete this section. Matsudaira gives a detailed description of the preparations for these *matsouri,* of the participants, and of the dances, with numerous quotations—some of considerable length—from the dialogue and chorus. The second part (pp. 77-132), is a consideration of the beliefs underlying and indicated by the festivals: the spirit (*tama* or *misaki*) which "inhabits the body that it animates and gives to it sensibility, intelligence, and the gift of dancing": the village deity in whose honor the villagers hold their festivals; the divinities who are summoned to be present; the half-gods (*e. g.,* Hino, king of fire, and Mizouno, queen of water) who figure in the performances. The third part of the work (pp. 135-168), treats of the position of the individual in the village economy and the organization of the village under religious and secular leaders as shown in these fetes.

From occasional mention in the text, as well as from the introduction and conclusion, it is clear that this was written primarily to supplement Hayakawa's work on the *Hanamatsouri* (Tokyo, 1930), a work of great value, in Matsudaira's judgment, for its encyclopedic wealth of information on the dances and their history, but one which neglected matters of equal or greater value from the sociological point of view. The reviewer has not seen Hayakawa's book, but it seems a reasonable supposition that the first section of *Les Fêtes Saissonières* is, in some measure, a briefer parallel account, the purpose of which is to lay a foundation on which the author may erect the superstructure, his analysis of the beliefs indicated by the festivals and the organization of the village. The conclusions that he reaches may be briefly summarized. (1) The development or growth of the individual soul from the moment when it first animates the body to the time when it leaves this earth is affected by ceremonies performed in connection with these festivals, and the relation between the growth of the soul and the

ceremonies is set forth in the dances and the dialogues. (2) Each of the villages studied has several Buddhist and Shinto temples; in general, one of the Shinto temples is the dwelling place of the village deity. In antiquity the deity was thought to inhabit some sacred tree, at the foot of which the villagers held their fetes. Later, when the tree died or was cut down, the wood was made into a shrine, and the festivities naturally enough were continued before the shrine or, yet later, about the temple that was constructed near it. (3) All the gods of the universe are invoked to take part in a reunion, by magic ceremonies, offerings, music, and dances; so the festivals are performances not merely in honor of the local deity but calculated to win the favor of an entire pantheon. (4) The organization and life of the villages is of such sort that individualism is quite inconceivable; the family, not the individual, is the unit.

In all this, there does not seem to be anything strikingly original. Dr. Matsudaira's study is well documented; he justly calls attention to the wealth of Japanese folklore, justly points out that it is a field that still offers opportunities for research, gives us a clear and well-reasoned account of certain ancient popular festivals and their background. It may be ungracious to find fault with him for doing no more, for evidently that is all he intended to do. The criticism that the present writer would offer it this: Matsudaira, like some other Japanese scholars dealing with the myths and folklore of Japan, gives no hint nor indication that Japanese mythological concepts are by no means unique, that similarities and parallels can readily be found. In other words, such a study as this would be more valuable, both in its descriptions and in its sociological analysis, were it so widened in scope as to find room for a comparative treatment of the matters with which it deals. Yet this criticism in no wise implies that Dr. Matsudaira has failed to perform thoroughly and satisfactorily the task which he set himself and which was undoubtedly worth doing.

Une Poetesse Japonaise au XVIII^e siècle, Kaga No Tchiyo-Jo. Par Gilberte Hla-Dorge. Paris: G.-P. Maisonneuve, 1936.

This is an unusually interesting work on Japanese poetry. A decade ago, or thereabouts, Mme. Hla-Dorge, then Mlle. Dorge, was working at the Sorbonne on the history of Japanese civilization and at the École des Langues Orientales on the Japanese language; in writing this doctoral thesis she has shown the advantage of possessing not alone mature scholarship but also a sympathetic insight into the thought and expression of the Japanese poets of the eighteenth century. So far as Japanese poetry is concerned, she points out, quite justly, that while the Japanese are in the main a nation of imitators (with, it should be added, a remarkable facility at assimilation) their poetry is sui generis; it may, and occasionally it does, contain echoes of Chinese thought, but the form of Japanese poetry is entirely other than that of the Chinese. The simplicity, brevity, and directness which are dominant characteristics of Japanese poetry are particularly clear in the form of versification called *haikai,* a poem of seventeen syllables. Of the poets and poetesses who excelled in this form none is more famous among the Japanese than Tchiyo (Chiyo would be the normal transliteration, but Dr. Hla-Dorge explains in her introduction that she writes with French readers in mind), some five hundred of whose poems are given, with translation, in this volume.

Kaga-no-Tchiyo was born in 1703 in a village within a few miles of Kanazawa. Her father was a maker of *kakemono,* and as a small child she played in the workshop where he was employed, saw there the works of art and the specimens of calligraphy which it was his task to mount. When she was nineteen years of age she was married to a retainer of the daimyo of Kaga; when he died ten years later she became a Buddhist religieuse, a consecrated widow living not in a convent but in her own house. In the year 1776 she died.

Such is the slender biographical framework about which this study is constructed. What Dr. Hla-Dorge has given us is a literary biography, with an introductory chapter on Japanese poetry and another on women of letters who composed *haikai.* Not till page 72 does Tchiyo step upon the stage. From there on a somewhat discursive narrative traces Tchiyo's poetical career and gives a vast deal of information regarding the ideas and ideals of her time.

For those who wish to penetrate beneath the surface in an endeavor to understand the spirit of old Japan in its fairer aspects, this book can be safely, and highly, recommended.

ALFRED H. SWEET.

Le Vieux Japon: "La Route de la Mer orientale." By MICHEL RIBAUD. Voyage du Jeune Stanislas au Japon, ou Essais sur la Civilisation japonaise—Histoire, Religion, Philosophie, Littérature & Beaux-arts. Volume 11. Paris: LIBRAIRIE D'AMÉRIQUE ET D'ORIENT, 1935. Pp. 256.

The first volume of the series "La Traversée, Tokio" was an introduction to the cultural bcakground of the entire Far east; hence the author's suggested title, "Dicours sur la Civilisation asiatique," is very apt. The present volume, the second in the series of four volumes, deals with the evolution of the island empire and traces its development from the earliest known date to the beginning of the Tokugawa regime (1603). Leaving the modernized city of Tokyo, the caravan journeys southward on the Tokaido highway, which for centuries was the main artery. Various post-stations such as Okitsu, Hamamatsu, and Nagoya provide excellent background for the legends and myths such as the story of the "Feather Robe" of the *No* play and the odyssey of Urashima. The pilgrimage to Ise opens up the history of Shintoism; the ancient city of Nara gives an occasion to narrate the introduction of Buddhism and Chinese culture, the new impulse to creative arts, and the subsequent rise of the native language and culture.

M. Ribaud incorporates an amazing amount of information in his small volume, and the journey is made exciting not only with history and ancient folk-lores), but also with keen observation of modern every day life. A charming dinner party, a tour through a modern porcelain factory, and the life and belief of young Japanese are skilfully woven in the narrative. Moreover, his discourse on civilization fits in well with the geographical features. His chapters on the early feudal regime and the spread of Christianity are slightly out of place, and if Mr. Ribaud is rather dogmatic on such controversial subjects as ethnology and anthropology, it is due to the nature of the book. On the whole the book is extremely informative and stimulating.

Les Notes de Chevet de Sei Shônagon, dame d'Honneur au Palais de Kyôto. (Traduction in extenso de l'ancient texte japonais). By ANDRÉ BEAUJARD. Paris: LIBRAIRIE ORIENTALE ET AMÉRICAINE, 1934. Pp. xxii, 329.

It seems that every Western scholar who went into Japanese literature tried his hand at the *Makura no Sôshi* of Sei Shônagon, but "because the original was dull, unintelligible, or so packed with allusion that it required an impracticable amount of commentary," only a quarter has been translated. Now for the first time M. Beaujard translates it in its entirety with copious notes. He followed the excellent commentary of Kaneko Motoomi, first published in 1924, and also Mizoguchi Hakuyô's translation into modern Japanese, issued in 1927. The parts which the Western scholars had thought dull and repetitive in the present translation become thoroughly alive with historical interest, and his notes are invaluable for the students of ancient Japanese literature.

The *Makura no Sôshi* is a plain record of facts, and consists partly of reminiscenes and partly of entries in diary-form. The book is not arranged chronologically, but under a series of headings, though often this scheme breaks down and the sequence becomes entirely arbitary. The authoress, Sei Shônagon was born in 966 or 967, the daughter of Kiyohara no Motosuke, whose ancestors distinguished themselves for their devotion to learning and literature. Shônagon entered the service of the Empress Sadako at the age of twenty-four or twenty-five and remained in the service for ten years till the Empress died. It is with these years, from 991 to 1000, that the *Makura no Sôshi* deals.

The present translation is done with the utmost accuracy and M. Beaujard has not only caught the author's unique prose style but also penetrated the aesthetic and literary civilization of the time. On the more debatable passages in the text, I refer the students to the more recent edition of the diary by Ikeda Kikan in the *Iwanami Library* in two volumes.

SHIO SAKANISHI.

Library of Congress.

Sei Shônagon, son Temps et son Oeuvre (une femme de lettres de l'ancien Japon). Avec une préface de M. Michel Revon. By ANDRÉ BEAUJARD. Paris: LIBRAIRIE ORIENTAL ET AMERICAINE, 1934. Pp. 377.

This is a companion volume to *Les Notes de Chevet de Sei Shônagon,* and it developed out of a set of notes accumulated by the author in his attempts to clarify and explain the obscure allusions and the conceptions in Sei Shônagon's memoirs. Written primarily as a guide to the reader of *Les Notes de Chevet,* it will serve many a social historian of the occident as a well documented source of material connected with life in the 10th century Japanese court.

The book is divided into three parts: Part 1 provides as general background a rapid survey of Japanese and Chinese history, the geographical divisions of the country, the political organization, the common religious beliefs, as well as the customs, interests, and daily occupations of the members of the court. Part II is a detailed review of the same period, given up largely to lists of important persons and events. Part III presents a penetrating and sympathetic analysis of Shônagon's work and character. Extensive quotations of Japanese and Chinese poetry enable the author to illustrate in this section the cultural and intellectual ideas to which she and her associates were bred and show very specifically what sources provided much of their knowledge and what patterns formed their literary taste and style.

It is this third portion of the work which M. Beaujard's master, M. Michel Revon, marks out for special praise in his preface, and deservedly so. For while the reconstruction of the probable knowledge of a given period in such a way as to throw light upon a given character or event is not of itself a particularly original device, it is no slight feat to have gathered the almost purely literary evidence of the sort to which M. Beaujard was of necessity confined—evidence by its very nature remarkably subtle and obscured moreover by the passage of time—and then to have analyzed the material with such descrimination as to have focussed it upon a figure little known except for her memoirs, and upon those few associates, even less well known, with whom she was intimate. Thanks to his shrewd findings as to the contemporary knowledge of folk-lore,

sciences, history, and geography, M. Beaujard is able to present Shônagon against a background relatively rich. There she emerges as not so much a blue-stocking, as a woman of wit and of noticeable presence of mind in exhibiting what she knew.

It is fitting that the first comprehensive study of the background of Sei Shônagon's memoirs should have been the work of a Frenchman. The parallel between the courts of Emperor Ichijô and Le Roi Soleil springs immediately to mind. Who, moreover, learning to know well the kind of life which fostered the wit, the presence of mind, the conscious gravity, and the intellectual snobbishness of Shônagon, is not reminded of that culture responsible for certain Précieuses and femmes savantes?

LILLIAN E. KNOWLES.

Mass. Hospital School,
Boston.

NOTES OF THE SOCIETY

Dr. Rudolf Meyer Riefstahl died December 31, 1936.

Professor Moriz Winternitz, honorary member of the Society, died January 9, 1937.

NOTES OF OTHER SOCIETIES

The *Revue d'Egyptologie* will resume regular publication in the year 1937. All books for review and correspondence should be addressed to the Secretariat de la *Revue d'Egyptologie*, chez M. Raymond Weill, 4bis, rue Damesme, Paris (13e).